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FOUNDED BY

W. F. R. WELDON, FRANCIS GALTON AND KARL PEARSON

EDITED

IN CONSULTATION WITH SIR FRANCIS GALTON

BY

KARL PEARSON

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VOLUME VII APRIL, 1910 No. 3

HISTOLOGICAL EXAMINATION OF A CASE OF ALBINISM.

BY J. E. ADLER, F.R.C.S. AND J. McINTOSH, M.D.

(From the Bacteriological Laboratory, London Hospital, E.)

ALBINISM has now long been a subject of interest to medical science, and many papers have appeared from time to time descriptive of its clinical peculiarities. The literature on the histological findings of the affected parts is, however, most scanty, and is usually confined to descriptions of pieces of the iris removed at operations for traumatic and other affections.

In looking through the literature only one record of a complete post-mortem examination on an albino could be found. This was made by Buzzi in 1783 (2). The case from his description appears to have been a complete albino, but his descriptions of the minute anatomy are far from satisfactory. Carron du Villards (1) and Maury (8) record dissections of human albino eyes; the former with absence of all pigment, the latter with traces of pigment.

It is a pity that a more uniform system of classification has not been adopted by the various observers in the description of their cases, so that it might have been possible to get a correct idea of the condition observed.

All the cases on which histological observations have been made, with the exception of Buzzi's case, would come under the groups of incomplete albinism, according to the classification proposed by Geoffroy Saint Hilaire (3).

In the case of partial albinism recorded by Mueller (9), there was a complete absence of all pigment in certain areas of the iris. In Nettleship's case there was a complete absence of pigment in the portion of the iris examined which was probably a fair sample. On the other hand, in Usher's cases of incomplete albinism recorded by Nettleship (10), there was diminished pigment in both choroid and retina. Clinically Usher's cases might pass for complete albinos, and this would mean that we could not distinguish by clinical methods between cases of incomplete and complete albinism.

The case with which we are about to deal, and which we had the opportunity of examining, was that of a female child of 10 weeks. The child was the second albino in the family, and the mother had taken the matter so much to heart that

she committed suicide. She left the child exposed, and from the effects of this it died.

Family History. The family history of the child was specially investigated in order to ascertain whether there was a hereditary history of albinism; but no evidence of albinism could be found in any of the ancestors or relations in the four previous generations. The description of the family tree (see p. 243) connected with the case is as follows (unless marked as albino, all members are normal):—

Generation IV. 12. B. C., aet. 10, albino—pink eyes, nystagmus, white hair, big healthy boy, very bright. 13. L. C., aet. 8, ♂ healthy. 14. —. C., aet. 6, ♀ healthy. 15. N., aet. 4, ♂. 16. O. C., aet. 3, ♀ healthy. 17. C. C., aet. 6 weeks, ♀ albino, the case described. IV. 1. S. W., aet. 25, ♂ healthy. 2. L. W., aet. 24, ♂ healthy. 3. E. W., aet. 22, ♀ healthy. 4. B. W., aet. 22, ♀. 5. P. K., aet.?, ♂. 6. A. K., aet.?, ♀. 7. G. D., aet. 13, ♂. 8. D. E., aet. 8, ♂. 9. J. E., aet. 9, ♂. 10. S. E., aet. 10, ♀. 11. M. E., aet. 11, ♀. 18. B. S., aet. 9, ♂. 19. D. S., aet. 7½, ♀.

Generation III. 1. R. W., father of family IV. 1—4. 2. M. A. W., wife of III. 1, healthy (eldest sister). 3. A. K., sister of III., 2. 4. P. K., her husband, father of family IV. 5—6. 5. L. C., mother of IV. 7, and wife of III. 6. 6. G. D., husband. 7. L. C., ♀, died. 8. E. C., ♀, died. 9. D. C., ♂, married to III. 10. 10. S. E., ♀, became wife of III. 9, mother of IV. 8—11. 11. A. C., ♂, died. 12. A. C., ♂, youngest child, father of case, married III. 27 (L. R.), alive. 13—17, Children of W. C., II. 3—4, all unaffected, but all details unknown. 18, 19, 20, and 21, Children of A. C., II. 5—6, all unaffected, details unknown. 22. A. R., ♀, spinster, pale, but not a true albino. 23. F. R., ♀. 24. G. R., ♂, in asylum. 25. H. R., ♂, married D. S., ♀, III. 26. ♀, wife of IV. 18, 19. 27. ♀, L. R., married A. C., III. 11, died examined, found unaffected, mother of the two albinos in the family. 28. ♂, S. R., just married to III. 29. ♀. 30. ♀, P. R., just married to III. 31. ♂, B. 32. If any, and how many, unknown. 33—34. Offspring of II. 11 and 12, age and sex unknown, known however to be normal. 35. If any, no details known. 36—45. Offspring of II. 15—16, all unaffected, but no details as to order and sex.

Generation II. 1. D. C., alive, ♂, aet. 74, unaffected. 2. M. A. S., ♀, wife of above, unaffected, parents of III. 1—12. 3. W. C., ♂, died, age unknown, unaffected. 4. ♀, his wife, unaffected. 5. A. C., ♂, died, age unknown, unaffected. 6. ♀, his wife, unaffected. 7. A. C., ♀, married G. R. (8), ♂, parents of family III. 22—30. 9. S. C., ♀, married 10. ♂, age and initials unknown. 11. L. C., ♀, married 12, ♂, age and initials unknown. 13. A. C., ♀, married 14, ♂, age and initials unknown. 15. M. C., ♀, married 16, ♂, age and initials unknown; II. 15—16 parents of III. 36—45. 17. ♀, sister of G. R., II. 8, unaffected. 18. ♀, sister of G. R., II. 8, unaffected. 19. ♀, sister of G. R., II. 8, unaffected.

Generation I. 1. D. C., ♂, died, aet. 72, unaffected. 2. ♀, wife of above, unaffected, all other details unknown. 3. R., ♂, unaffected, all details unknown. 4. ♀, unaffected.

Pathological Anatomy—Post-mortem Notes.

External Appearances. The body was that of a well nourished female child of about six weeks. Rigor mortis was absent.

There were small papular erythematous patches on the extensor aspects of both wrists, forearms and legs; symmetrically arranged ecchymoses were seen on the upper and inner aspects of both thighs.

The back, shoulders, and sides were covered with fine white downy hairs. The nipple, umbilicus, perineum, and anus were unpigmented. The scalp was covered with a large quantity of fine silky hair about an inch long. No hairs were pigmented. The eyebrows consisted of white hairs, while the eyelashes were regular, even, and white.

The eyes were pink; the pupils equal, and moderately small. The irides were entirely devoid of all pigment, and were pinkish in colour, with fine greyish white radially arranged striations.

Weight, 7 lbs. 14 ozs. Circumference of head, 14½ ins.

On opening the thorax, the thymus gland was found to be enlarged, and reached down as far as the level of the third costal cartilage; weight, ½ oz. There was no excess of fluid in either pleural cavity, and there were no pleuritic adhesions. The lungs were collapsed, definite capillary bronchitis being present. The mucous membrane of the bronchi was congested, and the larger bronchi contained some yellowish pus. There was no consolidation. Weight, right lung, 1¼ ozs.; left lung, 1¾ ozs.

The Heart. There was no excess of fluid in the pericardium. The foramen ovale was not completely closed, and admitted a fine probe. The valves and aorta were normal in appearance. Weight, ¾ oz.

The Stomach. The stomach was almost empty, and very pale in colour; two or three ecchymotic spots were noticed on its anterior surface near the greater curvature. The stomach contained about two or three drachms of mucous fluid, which was odourless. The mucous membrane appeared healthy.

The Liver protruded one finger's breadth below the costal margin, and, on being cut into, appeared to be normal. No thickening of the capsule was noticed. The gall bladder was filled with the usual yellow bile, and the ducts were found patent.

The Spleen (¼ oz. in weight) was firm in texture. On section, the malpighian bodies were not distinctly marked.

The Kidneys (¼ oz. each in weight) were lobulated. Their capsule stripped readily, and the perirenal fat was plentiful and normal in colour. The ureters were patent, and were followed into the bladder. The latter was empty, and was healthy in appearance. Both suprarenal glands were present, and appeared normal.

The small intestine was, for the most part, thin and empty, and the mucous membrane, though pale in colour throughout, appeared to be healthy; while the Peyer's patches presented a normal appearance. The large intestine, appendix, and rectum were examined and found to be healthy. The pancreas appeared to be normal, both in regard to structure and weight; the duct of Wirsung was patent. The mesenteric, bronchial, cervical, and axillary glands were normal in size, and presented the usual appearance on section. The tongue, tonsils, and

oesophagus were normal. The uterus and cervix were infantile in shape, but the ovaries were both enlarged and cystic. Pieces were removed from each organ, and placed in a solution of formalin (1 in 10) for histological examination.

The Brain was removed intact (weight, 1 lb. ½ oz.), and was very soft. The dura mater, as is usual in children, was adherent at the suture lines. The cerebral sinuses were found to be normal. The pituitary body with the sella turcica appeared to be normal, and were removed en masse.

Histopathology. All the organs and tissues were examined microscopically, but, apart from a complete absence of any pigment in certain parts, no histological peculiarities were observed. With regard to the liver, spleen, lungs, kidneys, pancreas, thymus, thyroid, heart, and muscles, no abnormality was made out; while in the other structures, namely, brain, eyes, internal ear, suprarenal gland, skin, and hair, the only noticeable feature was a complete absence of pigment.

Brain. Sections of the cerebrum, cerebellum, crura cerebri, pons Varolii, and medulla oblongata were examined, both in the stained and unstained condition. Search was made for pigment in those parts in which pigmentation normally exists in the brain cells in the adult. The various nuclei of the pons, medulla, and cerebellum, the substantia nigra and the grey matter were examined, but contained no pigment granules.

Sections of these areas were also stained with fluorescein, but no granules were found.

The pituitary body was apparently normal in structure.

The Eyes. Both eyes were examined completely, one was fixed in formalin (10 °/₀) and the other in spirit, and then they were frozen solid and cut into right and left halves. The internal aspect of the eyeballs was like yellowish white parchment. The vessels were clearly outlined on their surface, while the fovea centralis appeared as a distinct spot of a slightly deeper tint than the surrounding retina.

Paraffin and celloidin sections were made and examined under the microscope by day and artificial light, without even a trace of pigment being found in any of the structures of the eye, whether of an ectodermic or mesodermic origin, retina, epithelium and stroma of iris or choroid : see Plate II.

Owing to the entire absence of pigment in the eyes, the anatomical structure of the irides and retina was shown to great advantage, while the arrangement of the delicate connective tissue of the iris and its continuity with the choroid and ciliary processes were clearly seen. The arrangement of the two sets of muscle fibres could be followed. The iris also showed a well-marked crypt. Unfortunately the retina had not been fixed soon enough after death to preserve the rods and cones layer intact, their situation however can be easily recognised.

The choroid and retina were examined in the stained and unstained condition.

Internal Ear. The petrous portion of the temporal bone containing the middle and internal auditory apparatus was removed entire and decalcified. The internal ear contained no pigment in the mesoblastic cells of the perilymphatic spaces.

The Suprarenal Gland. The medulla and the cortex appeared to be of the usual proportions; no brown granules were to be observed in those cells of the cortex which are usually found to contain pigment in adult life.

The Skin. Except for the absence of pigment, the only noticeable feature seen in sections of the skin was the non-granular appearance of the rete mucosum.

The Hair contained no pigment; after the hairs were cleared in xylol they became quite transparent, and could be seen only with considerable difficulty.

In conclusion, one must say that the case described above is one of complete albinism, on account of the complete absence of any melanin pigment in those structures which normally contain it.

This child, though it might also be described in Professor Karl Pearson's terminology as an internal albino, does not solve the question as to whether internal albinism exists in adult albinos, because practically no pigment exists in any part of the brain of a child before the sixth month of life (Marinesco (7)). The same is also true for those glands, such as the suprarenal, which contain pigment in adult life (Koelliker (5)).

There was no evidence of any maldevelopment or mental feebleness in any member of the family, such as has been reported in certain albinotic stocks. But though there is undoubted proof that in certain instances deficiency of pigment does go along with such conditions, we have no reason to suppose that there is a constant relation between them.

In all probability this child, had it lived, would have remained a complete albino even in spite of the fact that the colour of the iris in all infants darkens considerably during the first year of life, because it is very unlikely that pigmentation would occur after so long a period of complete absence, namely, over 10 months, as melanin pigment, according to Koelliker (4), makes its appearance in the retina towards the fourth week of intra-uterine life. The specimens kindly lent by Dr Low (6) showed distinct pigment in the retina at this period: see Plate I.

Where the albinism entered this family is not at all clear; if the condition is not a sport it is most likely that the condition was handed down from the (paternal) grand-parents, as the parents of the child were cousins. From a Mendelian view both parents might be described as carrying recessive albinism, but we have really no evidence from this single case to support this view.

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DESCRIPTION OF PLATES.

Plate I. Fig. 1. Section of Eye in 4th week embryo.

Fig. 2. Section of Eye in 10th week foetus.

We owe these sections to the kindness of Dr A. Low.

Plate II. Fig. 3. Section of retina of albino case.

Fig. 4. Section of retina of normal infant.

Fig. 5. Section of iris of albino case.

Fig. 6. Section of iris from normal infant.

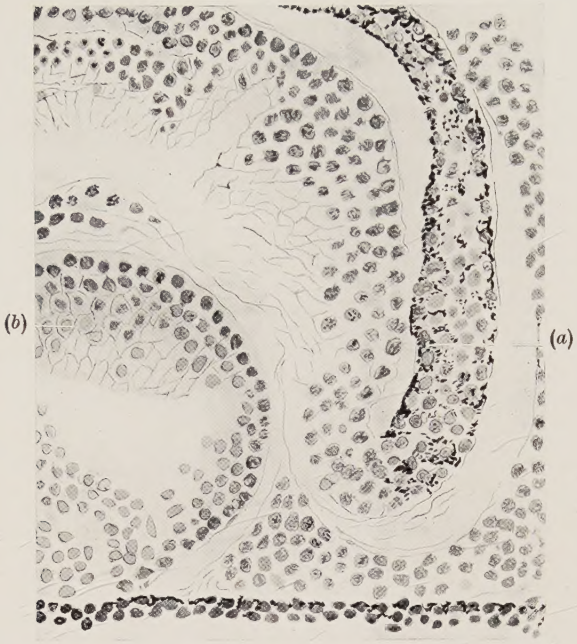

FIG. 1. Section of eye in 4th week embryo (10 mm. long).
(*a*) Pigment granules in pigment layer of retina.
(*b*) Lens.
[Dr Low's section]

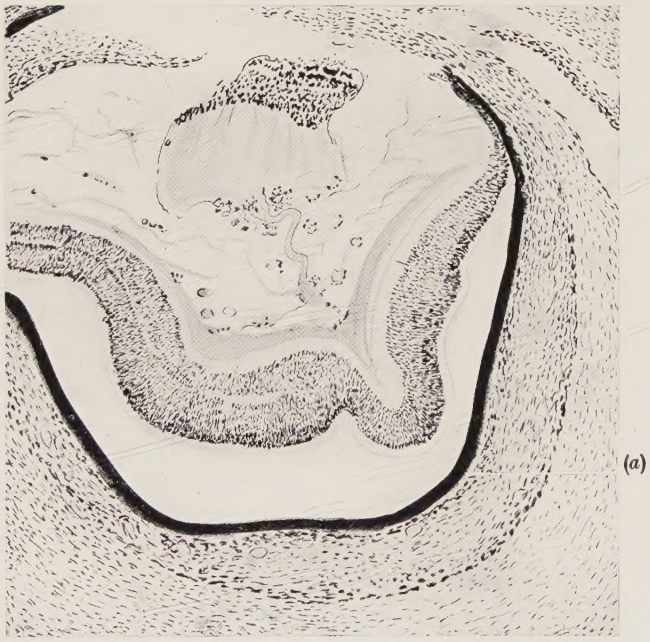

FIG. 2. Section of eye, 10th week foetus (28 mm. long).
(*a*) Pigment layer of retina.
[Dr Low's section]

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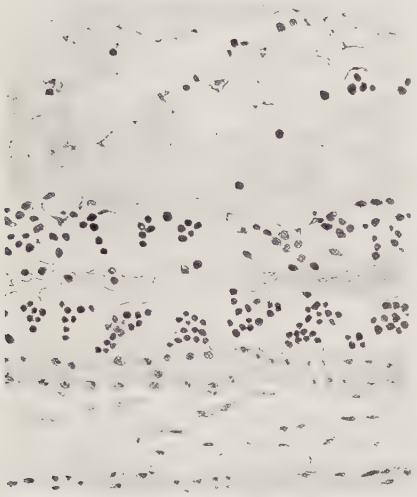


Fig. 3. Section of retina of albino child, with absence of all pigment.

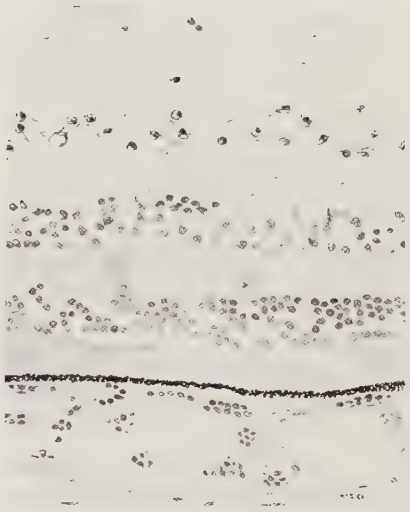


Fig. 4. Section of retina of normal child of same age, showing pigment layer.

Fig. 5. Section of iris of albino child, with absence of all pigment.

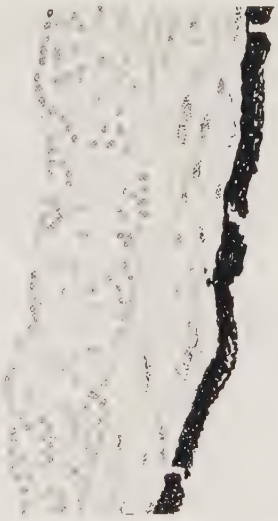
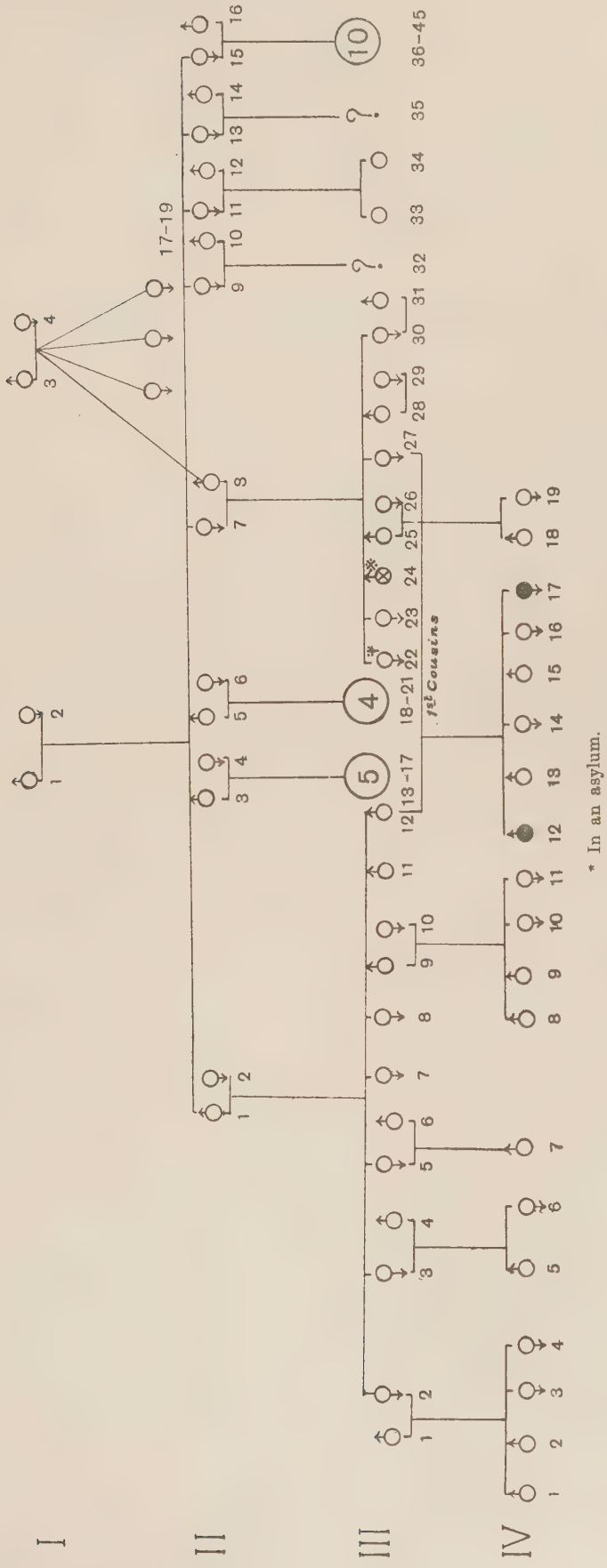


Fig. 6. Section of iris from normal child of same age.


I
II
III
IV
1st Cousins
* In an asylum.

NOTE ON INTERNAL ALBINISM.

By KARL PEARSON, F.R.S.

The authors of the previous paper having referred to my use of the term *internal albinism*, it may not, perhaps, be out of place to say a few words on that subject. This is not done because any conclusions on this point have been so far worked out, but because the investigations upon which they must turn will perforce have to be made by others with not only the adequate training but the requisite material,—a material which is rarely available, and which without such suggestions, as I wish to bring forward, may possibly not be used to the maximum advantage when it does occur. This must be my excuse for putting forward some rather vague suggestions instead of demonstrated facts; I hope that, among many other questions of interest, they may be partially answered when the opportunity arises.

With the exception of an occasional eye or part of an eye no post-mortem account of a human albino has appeared in print as far as I am aware between 1783 and 1910. Buzzi in 1783 had not the histological knowledge which would have enabled him to throw light on our modern problems. In Adler and M^{c}Intosh's case the age of the child was too slight for the case to give information as to the internal pigment of a fully developed human albino. In the case of the post-mortem examination recorded below the answers to several of my problems could have been given, but the problems were not in the mind of the medical man who made the examination. It is because such examinations must be very infrequent that I venture to throw out my queries and suggestions in the hope that they may reach physiologists and pathologists; and that although originating in a layman they may consider them worthy of investigation.

In the case of albinism in man there is no doubt of the largely relative character of the absence of superficial pigment*. Almost every grade of albinism, whether we consider the eyes, hair, or skin, occurs. But there is something more than graduated intensity of pigmentation; there may be local complete absences of pigment; besides incomplete albinism, we have partial albinism and this in a great variety of stages. Intensity and distribution of the superficial pigment

* The terms "superficial pigment" and "external albinism" are used to cover the visible pigment, or pigment exposed to the action of light. They include the retinal pigment although its morphological origin brings it into closer relation with the internal pigment of the brain centres.

varies immensely in man. Now besides the superficial pigment, there occurs in man internal pigment, notably in certain brain centres, internal ear, liver, kidneys, suprarenals, lungs, etc. It can scarcely be doubted that this internal pigmentation plays some part in the case of several of the organs mentioned, possibly a most important part in the case of the brain centres, the suprarenal glands, and the internal ear. Now is there any reason for supposing that this pigmentation varies like the superficial pigmentation in intensity and locality? If so, what are the signs, outward and visible, of incomplete or partial internal albinism? Where would it be most reasonable to look for the occurrence of such cases? It is noteworthy that pigment superficially injected into an albino does not remain on the surface, but is fixed by the suprarenals, liver, spleen and lungs; it is eliminated by the kidneys and intestinal epithelium. In Addison's disease, not only bronze areas but *leucotic* areas occasionally appear. It would seem reasonable therefore to suppose that there is not absolute independence between internal and external pigmentation; other cases could be cited, but the suggestion made is merely that it is not wholly unreasonable to look first for defects of internal pigment in those individuals or stocks where we find defects of external pigment.

Of course it is logical in the first place to demonstrate that internal pigment occurs in albinos at all. Adler and M^{c}Intosh's case throws no light from the human albino on this point; no more did Buzzi's. But Mr O'Donoghue* kindly investigated some albino rabbits, rats and mice for me and he assures me that internal pigment does occur in these as in normal rats and mice. We may assume that this is true for human albinos, but this is just one of the points which need elucidation as the occasion arises. However, the suggestion made is that it might be reasonable to look for defects, partial or incomplete of internal pigment, in the same stocks as we find albinism occurring in—either in the case of albinotic, or in the case of non-albinotic members.

I now pass to some special points. The partially albinotic cat, the cat with white hair and blue eyes, is invariably deaf. Recent histological examination of such cats has shewn that among other important deviations from the normal in the internal ear, the walls of the perilymph chamber lack pigmentation. We are not justified as yet in saying that this is related to the deafness, but the possibility that it is must be borne in mind. I am not aware of any careful histological examination of the pigment in the internal ear of congenital deaf-mutes. That some forms of congenital deaf-mutism arise from partial internal albinism is conceivable and worth inquiring into as opportunity arises.

* "I have found that the absence of pigment so noticeable in the external appearance of albinos does not seem to affect the internal organs. In examining the viscera of albino rabbits I found it impossible to discover any difference in colour that would enable them to be distinguished from those of normal animals. The spleen, liver, bile, kidney, adrenal body, Peyer's patches and salivary glands are to all appearances quite similarly coloured. Two rats and two mice, one of each pair being an albino and the other normal, gave similar results on examination and in addition the mesenteries of both normal and albino were alike in containing pigment cells whose presence was demonstrated by silver nitrate." Chas. H. O'Donoghue.

The disappearance of superficial pigment is one of the marks of senility, it affects the eyes and hair alike. Is it possible that this loss of superficial pigment can ever be accompanied by a loss of internal pigment? For example, possibly in the case of the brain centres? Senile imbecility and in its milder form mere senility might possibly be associated with a weakening of the intensity of pigmentation in certain of the brain centres. The bigger question now arises: Is it conceivable that any forms of imbecility, not all, but *any*, are associated with defective brain pigmentation? This opens up an exceedingly interesting inquiry for those who are able to examine the brains of imbeciles and the senile insane —what, if any, differentiation is there in the centres of brain pigmentation*? Ophthalmoscopic investigation shows a high percentage of incomplete albinism of the eye among the insane.

The Japanese mouse is usually a piebald with completely albinotic eyes, it possesses the singular habit of dancing, or better of spinning. This has been attributed to various causes. Is it wholly and absolutely certain that the spinning characteristic of this mouse may not also be associated with its partial albinism? A special type of human imbecile is the spinning idiot, who rotates like a top.

Fawn or red pigmentation occurs in close association with many types of albinism. Some types of red hair are, we have just found, wholly lacking in pigment granules. I have some reason for believing that a larger proportion of red hair will be found among congenital deaf-mutes than in the general population; it will in a forthcoming memoir on albinism by Messrs Nettleship, Usher and myself be shown to be much in excess in albinotic stocks. Examining albinotic stocks there appears, partly in the albinotic and partly in the non-albinotic members to be an excessive proportion of imbecility, idiocy and deaf-mutism†. It may well be that these associated defects are only stigmata of general degeneracy, but the possibility of their having some relation to absence of internal pigment is, I venture to think, worth bearing in mind. So much for the suggestions I wish to make; they at least suggest observations which can be made by those so fortunate as to be able to examine histologically cases of albinism, imbecility and deaf-mutism. They may be quite illusory but even an erroneous suggestion is often a useful guide in directing observation to unregarded points, and the authors of the above-mentioned memoir on albinism have sought in vain for any report on possible differentiation in the internal pigment of albinos—it does not appear to have been dealt with.

In the account which I give below of a third autopsy on an albino, the subject was a boy, who combined deaf-mutism and spinning idiocy with albinism. Any two of these characters would on the theory of probability be an extraordinarily rare chance combination, but the existence of all three seems almost to demonstrate some interrelation in pathological origin. This deaf-mute spinning albino was the son of a deaf-mute mother. Dr Alfred Miller, the Medical Superintendent of the

* Dr G. E. L. Keyes, I am glad to say, has this investigation in hand.

† Actual statistics will be given in the memoir referred to above.

Warwick County Lunatic Asylum, to whom I owe a copy of the Asylum notes on this case, has made full inquiries as to the family history of the patient, but without success, this being no doubt due to the fact that the patient was illegitimate. I have cordially to thank Sir James Crichton-Browne, for mentioning that he had many years ago seen this boy, and suggesting inquiry at the Asylum.

Particulars as to a Deaf-mute Albino Idiot.

Admitted, July, 1852. "W. L. aet. 9. A congenital idiot. Is the natural son of a deaf and dumb mother. For some time after going to the Birmingham Asylum where he has been about 16 months he had frequent bleeding from the nose, and used to scream a good deal in the night. He is deaf and dumb; but not destitute of intelligence. He is very active, running and jumping about, climbing chairs, tables, etc., and has a habit of spinning like a top, though not nearly so much as formerly. He is of an affectionate disposition." There is nothing interesting related in the notes after this date. He is stated to have had frequent attacks of severe epistaxis—and generally to have been in poor health.

He died on Sept. 18, 1865.

"Post-mortem examination 23 hours after death. Body extremely emaciated, rigor mortis well marked. Skull thick and dense. Dura mater thickened and firmly adherent. Arachnoid slightly thickened. Convolutions of cerebrum adherent.

Cerebrum.—Middle lobes very prominent and the corresponding fossae at the base of the skull proportionately deep. Grey matter of convolutions firmer than usual.

Punctae sanguineae very numerous. About three drachms of fluid in each lateral ventricle. Choroid plexuses granular. Right optic thalamus eroded superficially along the tract of the taenia semicircularis and its substance soft and easily broken down. Anterior part of the floor of the 3rd ventricle of a gelatinous appearance and a buff colour. Pineal body soft and friable. Corpora quadrigemina soft and almost diffluent. Cerebellum.—A small patch of superficial softening on each side of the anterior part of the under surface. Olivary bodies small and ill-defined. Pleurae adherent throughout. Pericardium universally adherent externally. Lungs full of tubercular matter.

I certify that the apparent cause of death of the above-named W. L. was Phthisis (chronic)."

W. H. PARSEY, *Medical Officer.*

(Copied and forwarded to K. P. by Alfred Miller, Medical Superintendent, in letter of Jan. 25, 1909.)

ON A NEW METHOD OF DETERMINING CORRELATION, WHEN ONE VARIABLE IS GIVEN BY ALTERNATIVE AND THE OTHER BY MULTIPLE CATEGORIES.

By KARL PEARSON, F.R.S.

(1) In a recent paper* I have dealt with the case when one variable is given by alternative categories and the other proceeds by quantitative groupings, for example when a population whose ages are recorded is classed into anaemic and non-anaemic; or a population whose cephalic indices have been measured is classed into conscientious and unconscientious sections. The object of the present paper is to carry the idea involved in such double row correlation tables a stage further by supposing the variable classified into multiple categories to be purely qualitative. Such variables I have elsewhere spoken of as categoric variables, to distinguish them from graduated variables. As illustrations we may take the eye colour of an individual and the presence or absence of pulmonary tuberculosis. Here the eyes may be grouped into seven or eight classes, but we can only record the presence or absence of the disease; it is true that the immunity of the individual is almost certainly a graded variable, but we are not able at present to measure it, and must be contented with an alternative classification. Again, the presence of malaria and the skin tint is another illustration from the same field; for while skin tint is undoubtedly a graduated variable, no medical inquirer has probably the energy or time to do more than group into multiple classes—perhaps seven or eight—separated by certain skin tint mosaics.

Hitherto such double row contingency tables could only be reduced by using the fourfold process, originally published by me in the *Phil. Trans.* Vol. 195 A, pp. 1—4. Such a process has two disadvantages; it assumes that a graduated variate which follows the Gaussian law is at the bottom of both classifications; and further it requires us to make *one* fourfold grouping, where many are possible. The choice left to the operator is not unique, and different selections may modify somewhat the result. Even if the mean result of taking several divisions be adopted, we do not get rid of an arbitrary element in the process, and the labour may occasionally be excessive.

The fourfold process, absolutely necessary as it often is, should on the whole be reserved for those cases in which a fourfold division has arisen from the very nature of the data and not be applied to double row tables, where by using it we

* *Biometrika*, Vol. VII. pp. 96—105.

lose all the advantage of having one variable in multiple categories. It is further to be noted that in many cases it is impossible to suppose any adequately graduated variable behind the multiple categoric variate. For example, if we wish to find the relationship between crime and occupation, we may easily select 10 or 12 groups of kindred occupations, and discuss the number of convictions in each such group of individuals, but it would be unreasonable to suppose any continuous variable behind this grouping, while we might reasonably suppose a continuous variable behind the tendency to criminality. The present method has the advantage of giving a unique solution for double row contingency tables; it makes less appeal to hypothesis than the fourfold division method applied to such cases, and it is far more rapid in execution.

Finally, the necessity for some such method has been forced upon my notice by the great frequency with which such double row contingency tables have recently occurred in the work of those dealing with medical, sociological and criminological statistics in my laboratory.

(2) The theory of the method is very simple. Let y be the categoric variate in multiple classes, and x the alternative variate. We suppose x to be ultimately continuous, but by using y we do not suggest continuity. For such a system the correlation ratio, η, has a perfectly definite meaning, it is the ratio of the standard deviation of the weighted means of the y-arrays of x's to the standard deviation of the whole population; in symbols:

$$\eta^2 = \frac{S\{n_y(x_y - \bar{x})^2\}}{N\sigma_x^2} \quad \text{.....(i),}$$

where n_y is the number of individuals in any y-category; x_y is the mean x for this category and $\bar{x}$ and σ_x are the mean and standard deviation of the x's of the whole population N. This value of η, it is well-known*, must lie between 0 and 1; it becomes equal to r, the correlation coefficient, when the regression is linear. Further the mean standard deviation of the weighted arrays is known to be $\sigma_x\sqrt{1-\eta^2}$, a value which becomes small as η approaches unity, or x becomes absolutely defined by the y-category in which it lies.

Now let ${}_y\sigma_x$ be the standard deviation for the x's which fall into a special category y, then we may take:

$$\eta^2 = \frac{1}{N} S\left(n_y \frac{x_y^2}{{}_y\sigma_x^2} \frac{{}_y\sigma_x^2}{\sigma_x^2}\right) - \left(\frac{\bar{x}}{\sigma_x}\right)^2 \quad \text{.....(ii),}$$

since $$S(n_y x_y) = N\bar{x}.$$

Now we have just seen that the mean value of ${}_y\sigma_x^2 = \sigma_x\sqrt{1-\eta^2}$, we shall therefore assume that the distribution is sufficiently homoscedastic for us to replace ${}_y\sigma_x^2/\sigma_x^2$ by its mean value $(1-\eta^2)$. Hence:

$$\eta^2 = (1-\eta^2) S\left(\frac{n_y}{N} \frac{x_y^2}{{}_y\sigma_x^2}\right) - \left(\frac{\bar{x}}{\sigma_x}\right)^2,$$

* *Roy. Soc. Proc.* Vol. 71, p. 303. "On the General Theory of Skew Correlation and Non-linear Regression." *Drapers' Research Memoirs*, 1905. Dulau & Co.

and from this it follows that

$$\eta = \left[\frac{\frac{1}{N} S\left\{ n_y \left(\frac{x_y}{{}_y\sigma_x} \right)^2 \right\} - \left(\frac{\bar{x}}{\sigma_x} \right)^2}{1 + \frac{1}{N} S\left\{ n_y \left(\frac{x_y}{{}_y\sigma_x} \right)^2 \right\}} \right]^{\frac{1}{2}} \quad \text{.....(iii).}$$

This is the value of the correlation ratio, based, so far, solely on the assumption that the arrays are sufficiently homoscedastic to permit of our replacing the ratio ${}_y\sigma_x/\sigma_x$ by its mean value. There has been no hypothesis of linear regression, still less of a Gaussian distribution.

It now remains to consider on what hypothesis we shall determine $x_y/{}_y\sigma_x$ and $\bar{x}/\sigma_x$, which are not directly determinable from the data. We shall assume that the mean of each array of x's as well as the mean of the whole population may be found in terms of their respective standard deviations by tables of the probability integral. In other words we assume the distribution of the x-variable to be approximately Gaussian. Thus we find uniquely the correlation ratio of a double row contingency table by the following assumptions:

(i) The alternative variate is sufficiently Gaussian to admit of the means being found by tables of the probability integral.

(ii). The arrays are approximately homoscedastic. This latter condition is unnecessary, if the alternative variable be replaced by one grouped in *three* classes, for then the actual value of ${}_y\sigma_x/\sigma_x$ can be found for each array and we need not give it the mean value $\sqrt{1-\eta^2}$. With regard to the multiple categoric variable no assumption as to the nature of its distribution has been, or need be, made.

Illustrations.

(3) I will take first some illustrations from a forthcoming paper dealing with the relation of pigmentation to psychical characters.

(A) *Conscientiousness in Boys and Hair Colour.* The conscientiousness was classified as "keen" and "dull" with an intermediate class, which was found to be relatively very small and distributed accordingly between the two main groups. We have the following table:

Hair Colour.

Conscientiousness.	Red	Fair	Brown	Dark	Jet Black	Totals
Keen ...	59·5	441·5	433	354·5	20·5	1309
Dull ...	24·5	194·5	188	162·5	16·5	586
Totals	84	636	621	517	37	1895

We have, using Sheppard's Tables:

$$\bar{x}/\sigma_x = -\cdot 4980,$$

and for the arrays in order from red to jet black:

$$\bar{x}_1/\sigma_{x_1} = -\cdot 5485, \quad \bar{x}_2/\sigma_{x_2} = -\cdot 5077, \quad \bar{x}_3/\sigma_{x_3} = -\cdot 5166,$$
$$\bar{x}_4/\sigma_{x_4} = -\cdot 4837, \quad \bar{x}_5/\sigma_{x_5} = -\cdot 1359.$$

There is thus an almost continuous decrease in conscientiousness as we pass from red to jet black hair*. Or, there is a very slight but sensible relationship of hair pigmentation and conscientiousness. Using formula (iii) we find

$$\frac{1}{N} S\left\{n_y \left(\frac{x_y}{\sigma_y}\right)^2\right\} = \cdot 2515.$$

Hence
$$\eta^2 = \frac{\cdot 2515 - \cdot 2480}{1 \cdot 2515} = \cdot 00276,$$

and
$$\eta = \cdot 053.$$

The assumption that conscientiousness is a continuous variate approximately following the normal distribution appears a reasonable one.

(B) *Self-consciousness and Hair Colour.* The data for 1914 schoolboys are given in the following table:

Hair Colour.

	Red	Fair	Brown	Dark	Jet Black	Totals
Selfconscious ...	42·5	344·25	296·5	238	14·25	935·5
Unselfconscious ...	36·5	322·75	351·5	251·5	16·25	978·5
Totals	79	667	648	489·5†	30·5†	1914

Here $\bar{x}/\sigma_x = +\cdot 02816$, and for the arrays:

$$\bar{x}_1/\sigma_1 = -\cdot 0953, \quad \bar{x}_2/\sigma_2 = -\cdot 0404, \quad \bar{x}_3/\sigma_3 = +\cdot 1066,$$
$$\bar{x}_4/\sigma_4 = +\cdot 0346, \quad \bar{x}_5/\sigma_5 = +\cdot 0823.$$

These give
$$\frac{1}{N} S\left\{n_y \left(\frac{x_y}{\sigma_y}\right)^2\right\} = \cdot 0052,$$

whence we find
$$\eta = \cdot 066.$$

It will be seen that hair colour is more closely related to self-consciousness than to conscientiousness, the red and fair boys being more self-conscious than the dark haired boys—an experience which may I think be confirmed on adults. The

* The values of x reduced to a common unit σ_x are: $\bar{x}_1/\sigma_x = -\cdot 5477$, $\bar{x}_2/\sigma_x = -\cdot 5070$, $\bar{x}_3/\sigma_x = -\cdot 5159$, $\bar{x}_4/\sigma_x = -\cdot 4830$, $\bar{x}_5/\sigma_x = -\cdot 1357$.

† One boy was placed intermediate in hair colour between dark brown and jet black.

means of the arrays are, however, not so smooth* as in the former case and there is some sign of the regression curve being heteroclinal.

(C) *Handwriting and Eye Colour.* I have chosen this illustration because it admits of using formula (ii) directly. The material is represented in the following table:

Handwriting.

Eye Colour.	Very Good	Good	Moderate	Poor	Bad	Totals
Light ...	45	249	312·75	112·5	28·75	748
Medium ...	60	252	302·75	99	27·25	741
Dark ...	41·5	137·5	182	63	8	432
Totals	146·5	638·5	797·5	274·5	64	1921

Let us call the range of medium eye colour h and measure the means first from the boundary of light and medium (x) and then from the boundary of medium and dark (x'). We have†

$$\bar{x}/\sigma_x = \cdot2809, \quad \bar{x}'/\sigma_x = \cdot7558, \quad h/\sigma_x = 1\cdot0368,$$
$$\bar{x}_1/\sigma_1 = \cdot5039, \quad \bar{x}_1'/\sigma_1 = \cdot5731, \quad h/\sigma_1 = 1\cdot0770, \quad \sigma_1/\sigma_x = \cdot9626,$$
$$\bar{x}_2/\sigma_2 = \cdot2794, \quad \bar{x}_2'/\sigma_2 = \cdot7880, \quad h/\sigma_2 = 1\cdot0674, \quad \sigma_2/\sigma_x = \cdot9713,$$
$$\bar{x}_3/\sigma_3 = \cdot2737, \quad \bar{x}_3'/\sigma_3 = \cdot7448, \quad h/\sigma_3 = 1\cdot0184, \quad \sigma_3/\sigma_x = 1\cdot0180,$$
$$\bar{x}_4/\sigma_4 = \cdot2280, \quad \bar{x}_4'/\sigma_4 = \cdot7405, \quad h/\sigma_4 = \cdot9684, \quad \sigma_4/\sigma_x = 1\cdot0705,$$
$$\bar{x}_5/\sigma_5 = \cdot1276, \quad \bar{x}_5'/\sigma_5 = 1\cdot1504, \quad h/\sigma_5 = 1\cdot2780, \quad \sigma_5/\sigma_x = \cdot8112.$$

We see that the last column gives us the means of finding the ${}_y\sigma_x/\sigma_x$ of the formula (ii). Also we have alternative methods of determining η according as to whether we use the first or second column, i.e. x or x'.

Using the first column we find:

$$\eta^2 = \cdot0835 - \cdot0789 = \cdot0046,$$

or:

$$\eta = \cdot068.$$

Using the second column we find:

$$\eta'^2 = \cdot5754 - \cdot5713 = \cdot0041,$$

or:

$$\eta' = \cdot064.$$

The close accordance of these results speaks well for the application of the method to the present material. We may test it again, assuming that medium and dark eyes are classed together, and using formula (iii), we find in this case

$$\eta'' = \cdot070,$$

* Reduced to a common unit σ_x, they are $\bar{x}_1/\sigma_x = -\cdot0951$, $\bar{x}_2/\sigma_x = -\cdot0403$, $\bar{x}_3/\sigma_x = +\cdot1064$, $\bar{x}_4/\sigma_x = +\cdot0345$, $\bar{x}_5/\sigma_x = +\cdot0821$.

† Reduced to σ_x we have $\bar{x}_1/\sigma_x = \cdot4851$, $\bar{x}_2/\sigma_x = \cdot2714$, $\bar{x}_3/\sigma_x = \cdot2780$, $\bar{x}_4/\sigma_x = \cdot2441$, $\bar{x}_5/\sigma_x = \cdot1035$.

agreeing for all practical purposes with the value found for η above, and indicating that although the arrays as shown in the fourth column are heteroscedastic, it is practically sufficient to give ${}_y\sigma_x/\sigma_x$ its mean value. Examining Column 1, we see that the mean eye colour of the arrays shifts continuously towards the light as we pass from good to bad handwriters. The association is small, but can hardly be without significance.

(4) Alcoholism is a character which may be reasonably treated as one of graduated intensity, and one accordingly to which our method may be reasonably applied. I take as an illustration some statistics of crime most kindly provided by Dr Charles Goring.

(D) *Alcoholism and Type of Crime.* In the following table the crime categories have been arranged in the order of relative intelligence as ascertained from Dr Goring's correlation investigations on crime and intelligence.

Class of Crime.

	Arson	Rape	Violence	Stealing	Coining	Fraud	Totals
Alcoholic ...	50	88	155	379	18	63	753
Non-alcoholic ...	43	62	110	300	14	144	673
Totals	93	150	265	679	32	207	1426

We find: $\bar{x}/\sigma_x = +\cdot0704$, and for the arrays*:

$$\bar{x}_1/\sigma_1 = +\cdot0946, \quad \bar{x}_2/\sigma_2 = +\cdot2190, \quad \bar{x}_3/\sigma_3 = +\cdot2145,$$
$$\bar{x}_4/\sigma_4 = +\cdot1463, \quad \bar{x}_5/\sigma_5 = +\cdot1573, \quad \bar{x}_6/\sigma_6 = -\cdot5119.$$

Using formula (iii), we determine, $\eta = \cdot234$. There is thus quite a sensible correlation between the extent of alcoholism in the criminal and the type of crime he will commit †. Starting with fraud, there is increased alcoholism as we pass to stealing, coining, violence and rape. An exception, however, occurs in the case of arson, the class of crime whose perpetrators are least intelligent among criminals. Those who commit arson appear to be less alcoholic than coiners and thieves—criminals who require considerable sobriety for success in their callings. Probably the coiners would stand above thieves in the scale, did not the returns include the offenders who pass false coin as well as the producers; further, highly skilled but alcoholic workmen, who cannot retain on that account their berths, may in the case of certain trades tend to gravitate into this class of criminal. While rape and crimes of violence arise from a low grade of intelligence combined with alcoholism, it would seem that arson has a low grade of intelligence for its source, but probably some character such as revenge rather than alcoholism ultimately at its source.

* Reduced to a common unit: $\bar{x}_1/\sigma_x = +\cdot0920$, $\bar{x}_2/\sigma_x = +\cdot2129$, $\bar{x}_3/\sigma_x = +\cdot2085$, $\bar{x}_4/\sigma_x = +\cdot1422$, $\bar{x}_5/\sigma_x = +\cdot1529$, $\bar{x}_6/\sigma_x = -\cdot4977$.

† The reader must remember that this has no bearing on the problem of whether alcoholism leads to crime.

(E) *Alcoholism in Parents and Intelligence in the Offspring.* I owe the data for this illustration to Miss Elderton, who is treating alcoholism in parents and its effect on children at length in a forthcoming memoir. Some of her data are given in the accompanying table:

Intelligence of Sons (Edinburgh Data).

Parents.	Excellent	Good	Medium	Dull	Defective	Totals
Alcoholic ...	19	125	116	46	7	313
Non-alcoholic ...	23	97	99	45	9	273
Totals	42	222	215	91	16	586

We have: $\bar{x}/\sigma_x = -\cdot0857$, and for the arrays*:

$$\bar{x}_1/\sigma_1 = +\cdot1197, \quad \bar{x}_2/\sigma_2 = -\cdot1587, \quad \bar{x}_3/\sigma_3 = -\cdot0993,$$
$$\bar{x}_4/\sigma_4 = -\cdot0138, \quad \bar{x}_5/\sigma_5 = +\cdot1573,$$

this last value being of very small weight. The results are most remarkable; if we omit the excellent intelligences, we obtain an almost uniformly descending intensity of alcoholism in the parent, as the intelligence of the child diminishes; the least alcoholic parents have the less intelligent offspring. If we treat the table by a fourfold division, between "good" and "medium" intelligences we find for the correlation of parental sobriety and intelligence in offspring the value $r = -\cdot03$. It is clear that in the "excellent" group some special cause is at work, which gives the regression its non-linear character, and leads to a correlation ratio η of ·086. The same want of regular increase of intelligence of offspring with sobriety of parent is found in the case of parents and daughters, where the correlation coefficient is zero and the correlation ratio $\eta = \cdot102$, the "excellent" as well as the "dull" and "defective" groups of daughters having in this case the more alcoholic parents. It is clear that the relation of alcohol in parents to intelligence in children is not of a close kind and the association, such as it is, is not of a continuous character and apparently different in the two sexes.

(F) *Health of Daughters and Alcoholism of Fathers.* Miss Elderton has provided me with the following table:

Health of Daughters (Manchester Data).

Fathers.	Healthy	Fair	Delicate	Phthisical and Epileptic	Died Young	Totals
Alcoholic ...	104	10	24	8	38	184
Non-alcoholic ...	254	16	59	41	83	453
Totals	358	26	83	49	121	637

* Reduced to a common standard: $\bar{x}_1/\sigma_x = +\cdot1193$, $\bar{x}_2/\sigma_x = -\cdot1581$, $\bar{x}_3/\sigma_x = -\cdot0989$, $\bar{x}_4/\sigma_x = -\cdot0989$, $\bar{x}_5/\sigma_x = -\cdot0137$, $\bar{x}_6/\sigma_x = +\cdot1567$.

$\bar{x}/\sigma_x = +\cdot5567.$

Here* $\bar{x}_1/\sigma_1 = +\cdot5519$, $\bar{x}_2/\sigma_2 = +\cdot2934$, $\bar{x}_3/\sigma_3 = +\cdot5559$,
$\bar{x}_4/\sigma_4 = +\cdot9811$, $\bar{x}_5/\sigma_5 = +\cdot4844.$

The correlation ratio for this table is $\eta = \cdot133$, but a little examination shows that there is no definite association of poor health in child and alcoholism in father. Of the healthy children and of the delicate children the fathers have equal intensity of alcoholism; the phthisical and epileptic children come in an overwhelming proportion from the sober parents. The parents of those who die young are somewhat more alcoholic than those who survive. The small difference may well be due to more accidents and not to worse health in the children of alcoholic parents.

(F) *bis. Health of Sons and Alcoholism of Fathers.* Miss Elderton's table is as follows:

Health of Sons (Manchester Data).

Fathers.	Healthy	Fair	Delicate	Phthisical and Epileptic	Died Young	Totals
Alcoholic ...	122	9	24	8	42	205
Non-alcoholic...	328	37	71	37	133	606
Totals	450	46	95	45	175	811

Here† $\bar{x}/\sigma_x = +\cdot6658$, and for the arrays:

$\bar{x}_1/\sigma_1 = +\cdot6095$, $\bar{x}_2/\sigma_2 = +\cdot8573$, $\bar{x}_3/\sigma_3 = \cdot6662$,
$\bar{x}_4/\sigma_4 = +\cdot9239$, $\bar{x}_5/\sigma_5 = +\cdot7063.$

The correlation ratio is here $\eta = \cdot089$, but the correlation coefficient obtained from a fourfold table of healthy against the remainder is $-\cdot06$, or the children of alcoholic fathers are the more healthy. It is quite easy to see from the above results how little evidence there is for alcohol in fathers affecting the health of sons. As in the case of girls, but more sensibly so, the healthy children have fathers somewhat more alcoholic than the average; the delicate children have fathers not less sober than the average; the phthisical and epileptic children have fathers markedly more sober than the average population, and the early deaths are slightly more frequent among the children of sober parents. It is difficult to see in either of these tables evidence for alcohol as a chief source of bad health or poor intelligence in the offspring.

(5) As a last illustration I take certain statistics of the severity of attack in cases of smallpox and the strength of immunity provided by vaccination. I owe the following data to Dr John Brownlee of the City of Glasgow Hospital.

* Reduced to a common standard: $\bar{x}_1/\sigma_x = +\cdot5470$, $\bar{x}_2/\sigma_x = +\cdot2908$, $\bar{x}_3/\sigma_x = +\cdot5510$, $\bar{x}_4/\sigma_x = +\cdot9725$, $\bar{x}_5/\sigma_x = +\cdot4801$.

† Reduced to a common unit: $\bar{x}_1/\sigma_x = +\cdot6071$, $\bar{x}_2/\sigma_x = +\cdot8539$, $\bar{x}_3/\sigma_x = +\cdot6635$, $\bar{x}_4/\sigma_x = +\cdot9202$, $\bar{x}_5/\sigma_x = +\cdot7035$.

(G) *Severity of Attack and Strength of Immunity due to Vaccination.* I look first at the data arranged as a two row correlation table.

Severity of Attack.

Years since Vaccination.	Haemorrhagic	Confluent	Abundant	Sparse	Very Sparse	Totals
Since vaccination *0* to *25* years	5	38	120	176	148	487
Over *25* years or unvaccinated	44	251	388	308	211	1202
Totals	49	289	508	484	359	1689

$x/\sigma_x = +\cdot5583.$

Here* $\bar{x}_1/\sigma_1 = +1\cdot2700$, $\bar{x}_2/\sigma_2 = +1\cdot1194$, $\bar{x}_3/\sigma_3 = +\cdot7185$,
$\bar{x}_4/\sigma_4 = +\cdot3488$, $\bar{x}_5/\sigma_5 = +\cdot2218$.

From these results we find $\eta = \cdot321$, a very sensible relationship, and we see how the mean vaccination immunity increases as we pass from haemorrhagic to very sparse cases. There is very little doubt that we are dealing here with a perfectly graduated and continuous cause and effect, remarkably different in its character from the alcoholic influences we have just discussed.

To illustrate this point, I proceed to investigate the same data arranged as a three rowed table, using formula (ii). We have:

Severity of Attack.

Years since Vaccination.	Haemorrhagic	Confluent	Abundant	Sparse	Very Sparse	Totals
0—25 years ...	5	38	120	176	148	487
25—45 years ...	29	155	299	268	181	932
Over *45* or unvaccinated	15	96	89	40	30	270
Totals	49	289	508	484	359	1689

We have†: $\bar{x}/\sigma_x = \cdot9951$, $\bar{x}'/\sigma_x = \cdot5583$, $h/\sigma_x = 1\cdot5533$;
$\bar{x}_1/\sigma_1 = \cdot5069$, $\bar{x}_1'/\sigma_1 = 1\cdot2700$, $h/\sigma_1 = 1\cdot7769$, $\sigma_1/\sigma_x = \cdot8742$;
$\bar{x}_2/\sigma_2 = \cdot4329$, $\bar{x}_2'/\sigma_2 = 1\cdot1194$, $h/\sigma_2 = 1\cdot5523$, $\sigma_2/\sigma_x = 1\cdot0007$;
$\bar{x}_3/\sigma_3 = \cdot9338$, $\bar{x}_3'/\sigma_3 = \cdot7185$, $h/\sigma_3 = 1\cdot6524$, $\sigma_3/\sigma_x = \cdot9401$;
$\bar{x}_4/\sigma_4 = 1\cdot3875$, $\bar{x}_4'/\sigma_4 = \cdot3488$, $h/\sigma_4 = 1\cdot7363$, $\sigma_4/\sigma_x = \cdot8946$;
$\bar{x}_5/\sigma_5 = 1\cdot3815$, $\bar{x}_5'/\sigma_5 = \cdot2218$, $h/\sigma_5 = 1\cdot6032$, $\sigma_5/\sigma_x = \cdot9689$;

* Reduced to a common unit: $\bar{x}_1/\sigma_x = +1\cdot2028$, $\bar{x}_2/\sigma_x = +1\cdot0602$, $\bar{x}_3/\sigma_x = +\cdot6805$, $\bar{x}_4/\sigma_x = +\cdot3303$, $\bar{x}_5/\sigma_x = +\cdot2101$.

† Reduced to a common unit: $\bar{x}_1/\sigma_x = \cdot4431$, $\bar{x}_2/\sigma_x = \cdot4332$, $\bar{x}_3/\sigma_x = \cdot8779$, $\bar{x}_4/\sigma_x = 1\cdot2413$, $\bar{x}_5/\sigma_x = 1\cdot3385$.

leading, according to our horizontal division, to $\eta = \cdot 319$ and $\eta' = \cdot 337$ with a mean value ·328, in excellent agreement with the two row table value ·321.

Lastly the whole data has been taken as a contingency table:

Severity of Attack.

Years since Vaccination.	Haemorrhagic	Confluent	Abundant	Sparse	Very Sparse	Totals
0—10	—	1	6	11	12	30
10—25	5	37	114	165	136	457
25—45	29	155	299	268	181	932
Over *45*	11	35	48	33	28	155
Unvaccinated	4	61	41	7	2	115
Totals	49	289	508	484	359	1689

Worked out as a 25-fold contingency table by mean square contingency we have

$$C_2 = \cdot 335,$$

which lies between the two values of η found from the three rowed table by the new method, and is within the probable error of either. This agreement is very satisfactory evidence that it was legitimate to apply the new method to a case of this kind, and that in this particular instance the material was closely normal.

(6) The illustrations given will, I think, show that a wide range of problems can be dealt with by the new method, and that results found by it are closely comparable with results obtained by other processes. It does not assume linearity of regression, and in fact as in the examples on alcoholism it brings out in a very effective way the deviations from linearity. The hypothesis used is: that in the case of the variable with alternate categories, we can suppose it continuous and sufficiently normal in character to have the mean values found from tables of the probability integral. If the arrays are not sufficiently homoscedastic for us to replace their standard deviations by their mean value, then we must use if we can a three and not a two rowed table. It is always desirable to have if possible such a table, because it enables us to test the nature of the variability in the arrays. Our last illustration, however, shows that very considerable variations in the standard deviations of the arrays do not sensibly modify the result obtained by using for those standard deviations their mean value. On the whole this new method, which replaces the fourfold table method for cases in which the latter does not give a unique answer, is, I think, likely to prove of some service.

ON THE EFFECT OF A DIFFERENTIAL FERTILITY ON DEGENERACY.

By KARL PEARSON, F.R.S.

A NEW YEAR'S GREETING TO FRANCIS GALTON, 1910.

(1) I HAVE indicated in several earlier papers* the very marked changes which are produced when there is correlation between fertility and any character in a species. I have termed the changes which result in the mean and variability of the character *genetic or reproductive selection*, and have shown that if such correlation exists reproductive selection may wholly defeat or largely neutralise the results of natural selection. In actual wild life, it is very difficult to find any character sensibly correlated with fertility, and it would appear that the low value of correlations of this character is an essential condition for rapid progress by natural selection. Nature has in some way—at present not clear to us—prevented this reversal of the survival of the fitter by suppressing all *marked* correlation between the physical characters and the fertility of a species. Half a century ago, I think, this absence of correlation between fertility and physique would have been found to hold for man in this country. It is practically certain that it does not hold to-day. Artificially a differential fertility has been created; the better mental as well as physical characters can be shown to be associated with a lessened fertility and a reproductive selection has been called into play, which not only impedes, but possibly reverses natural selection. The object of the present paper is to obtain—at any rate to a first approximation—some measure of the secular changes in a race which must flow from a correlation of fertility with any character of an organism. The problem in the case of bisexual reproduction is not a wholly straightforward one. It is influenced in the first place by the intensity of assortative mating in the species. In the next place we have to ask: Does the fertility depend on the intensity of the character in one or in both parents? And lastly we must find a reasonable form for the relation between fertility and the intensity of the character. These points will be considered in the following sections.

* See *Phil. Trans.* Vol. 187, p. 258; Vol. 192, pp. 259, 314; *R. S. Proc.* Vol. 59, p. 303. *The Chances of Death*, Vol. I. p. 63 et seq.

(2) We will suppose that some character—e.g. grade of intelligence—influences the fertility. We have then to consider whether this character in the male and female influences equally or unequally the fertility of the pair. It has been asserted by some that academic training in the woman lessens her power of child-bearing. In this case the more intelligent women would have fewer offspring, but owing to assortative mating such women marry the more intellectual men, and whether intelligence was or was not associated with a lesser grade of fertility in the male, assortative mating would handicap the fertility of the intelligent male, and the more intelligent males would be practically less fertile.

Let ξ_1 and ξ_2 be the deviations from their respective sex means of male and female for any character, σ_1 and σ_2 the standard deviations with respect to this character in the two sexes, represented by the subscripts 1 and 2. Let r_{12} be the correlation of ξ_1 and ξ_2, i.e. the intensity of assortative mating.

Then it is reasonable to assume that the fertility of the pair is some function of x, where x is a linear function of ξ_1 and ξ_2, or:

$$x = c_1 \frac{\xi_1}{\sigma_1} + c_2 \frac{\xi_2}{\sigma_2} \quad \text{.....(i).}$$

The mean of x is clearly zero, and we can free ourselves from the influence of either parent by putting c_1 or c_2 zero. If fertility were related to x by a simple linear correlation then we should obtain, it is well known, the highest correlation of fertility (y) and x by taking (the subscript 3 denoting fertility):

$$c_1 = \frac{r_{13} - r_{12}r_{23}}{1 - r_{12}^2}, \qquad c_2 = \frac{r_{23} - r_{12}r_{13}}{1 - r_{12}^2} \quad \text{.....(ii),}$$

or proportional to these quantities. We need not at present however assume any special values for c_1 and c_2. Further, it is desirable to suppose that the curve of mean fertility for each value of x is not necessarily linear, but of a more general type, allowing us to make fertility a maximum at other grades than the extreme values of the character.

(3) This leads us to the next point. What law of fertility seems reasonable? We want a law of fertility which will allow closely of the fertility (a) increasing nearly uniformly and at any given rate with the character, (b) decreasing nearly uniformly and at any given rate with the character, and (c) being concentrated with any degree of intensity round any grade of the character, and falling away on both sides of this maximum. All these conditions are fulfilled if we assume the mean fertility y for any grade of the character x, not to be given by a line but by a normal curve, e.g.

$$y = y_0 e^{-\frac{1}{2}(x-k)^2/\sigma_0^2} \quad \text{.....(iii).}$$

Here by a proper choice of σ_0 and k, we can make the distribution of fertility fall or rise with the character (i.e. k positive or negative and both k and σ_0 very large,—k so taken that the centre of fertility lies outside the region of the range of values of x), or concentrate the fertility on any grade of the character (i.e. make

k equal that value, and take σ_0 small). It appears therefore that the distribution indicated, if not ideal, will still serve as a first approximation to the type of cases likely to occur.

There is, however, an argument of still greater weight for the suggestion that a fertility curve of this kind cannot be widely removed from the actual state of the case; namely, when we measure almost any human character (i) in parents and (ii) in their offspring we find both distributions very approximately normal. This could not possibly be the case unless the fertility curve were itself approximately normal. We include of course under this the case in which σ_0 is indefinitely great, or the fertility is not associated with the character in question, which is probably what we may describe as the *organic* condition of affairs. Of course in actual working we have to regard many special features. For example, in the matter of intelligence, it is fairly obvious that the abler men and women have fewer children; but is it true that the fertility falls again towards the extremely dull? This is a point which needs careful consideration; I think it probable, especially when we deal with net and not with gross fertility. Extreme mental defect leads to neglect of offspring, to life in institutions and limited marriage. For example, pauper fertility is higher than criminal fertility. I shall illustrate later this point.

(4) Let us now turn to the direct problem. A parentage has the deviation x of the character and σ_x is the variability of this deviation; the deviation in the offspring is z with a variability σ_z and a correlation ρ. Then, if there were no weighting with a differential fertility, the frequency surface would be

$$u = u_0 e^{-\frac{1}{2}\frac{1}{1-\rho^2}\left(\frac{x^2}{\sigma_x^2} - \frac{2\rho xz}{\sigma_x\sigma_z} + \frac{z^2}{\sigma_z^2}\right)} \quad \text{.....(iv)}$$

in the usual way. But we must multiply this by the average fertility of each grade to obtain the frequency distribution. This will be

$$v = v_0 e^{-\frac{1}{2}\frac{(x-k)^2}{\sigma_0^2}} \times e^{-\frac{1}{2}\frac{1}{1-\rho^2}\left(\frac{x^2}{\sigma_x^2} - \frac{2\rho xz}{\sigma_x\sigma_z} + \frac{z^2}{\sigma_z^2}\right)} \quad \text{..... .....(v)}$$

$$= v_0 e^{-\frac{1}{2}\frac{k^2}{\sigma_0^2}} \times e^{-\frac{1}{2}\frac{1}{1-\rho^2}\left\{x^2\left(\frac{1}{\sigma_x^2} + \frac{1-\rho^2}{\sigma_0^2}\right) - \frac{2xk(1-\rho^2)}{\sigma_0^2} - \frac{2\rho xz}{\sigma_x\sigma_z} + \frac{z^2}{\sigma_z^2}\right\}} \quad \text{...(v) } bis$$

$$= v_0' \times e^{-\frac{1}{2}\frac{1}{1-R^2}\left\{\frac{(x-m_1)^2}{\Sigma_1^2} - \frac{2(x-m_1)(z-m_2)R}{\Sigma_1\Sigma_2} + \frac{(z-m_2)^2}{\Sigma_2^2}\right\}} \quad \text{.....(vi),}$$

where v_0' is a fresh constant, i.e. independent of x and z, and m_1 and m_2 are the means of effective parentages (i.e. weighted parentages) and of actual sons, Σ_1 and Σ_2 their standard deviations and R their correlation. We have by finding the centre of the elliptic contour of (v) *bis* in the usual way:

$$m_1 = k\sigma_x^2/(\sigma_0^2 + \sigma_x^2) \quad \text{.....(vii),}$$

$$m_2 = m_1\rho\sigma_z/\sigma_x \quad \text{.....(viii).}$$

The first of these equations shows the shifting of the parental mean, the change being due to differential fertility; the second equation shows that the shift of the

offspring mean exactly follows the shift of the parental mean, or the mean of the offspring is the offspring of the mean parent. This gives at once the secular change due to weighting the parentages with their fertility.

Further we must have

$$\frac{1}{(1-R^2)\,\Sigma_1^2} = \frac{1}{1-\rho^2}\left(\frac{1}{\sigma_x^2} + \frac{1-\rho^2}{\sigma_0^2}\right),$$

$$\frac{1}{(1-R^2)\,\Sigma_2^2} = \frac{1}{1-\rho^2}\left(\frac{1}{\sigma_z^2}\right),$$

$$\frac{R}{(1-R^2)\,\Sigma_1\Sigma_2} = \frac{\rho}{1-\rho^2}\frac{1}{\sigma_x\sigma_z}.$$

From these flow readily

$$R = \frac{\rho}{\sqrt{1+(1-\rho^2)\dfrac{\sigma_x^2}{\sigma_0^2}}} \qquad \text{(ix)},$$

$$\Sigma_1 = \frac{\sigma_x}{\sqrt{1+\dfrac{\sigma_x^2}{\sigma_0^2}}} \qquad \text{(x)},$$

$$\Sigma_2 = \sigma_z\sqrt{1-\frac{\rho^2}{1+\dfrac{\sigma_0^2}{\sigma_x^2}}} \qquad \text{(xi)}.$$

These results show us at once that differential fertility: (i) reduces the variability (Σ_2) of the offspring, (ii) reduces the variability of the effective parentages, (iii) reduces the apparent correlation between parentage and offspring. It should be noted that the non-differential fertility is obtained by putting $\sigma_0 = \infty$; thus any differential fertility up to $\sigma_0 = 0$, which denotes breeding from a single grade, will give lowered values for R, Σ_1 and Σ_2.

While the variabilities and correlation are thus changed, it is worth noting that the regression of offspring on parents, i.e. $R\Sigma_2/\Sigma_1 = \rho\sigma_z/\sigma_x$ and is therefore unchanged. Further the equation to the regression line is

$$z - m_2 = \rho\frac{\sigma_z}{\sigma_x}(x - m_1),$$

or since $m_2 = m_1\rho\sigma_z/\sigma_x$, it follows that

$$z = \rho\frac{\sigma_z}{\sigma_x}x$$

or is identical with the regression line for non-differential fertility. This is, of course, only a case of the general principle found long ago* that selection of a character A does not alter the regression of a second character B on A, but it is interesting to note that the effect of differential fertility is as it were a pushing of the population down its regression line.

* *Phil. Trans.* Vol. 197, pp. 20, 21.

(5) We may next inquire into the mean fertility and variability of fertility in the population.

The fertility of the parentage x is

$$y = y_0 e^{-\frac{1}{2}(x-k)^2/\sigma_0^2} \quad \text{.....(iii),}$$

and the frequency of such parentages is given by

$$w = \frac{N}{\sqrt{2\pi}\sigma_x} e^{-\frac{1}{2}x^2/\sigma_x^2}$$

where N is the total number of such parentages.

Hence, if $\bar{f}$ = the mean fertility:

$$N\bar{f} = \int_{-\infty}^{+\infty} \frac{Ny_0}{\sqrt{2\pi}\sigma_x} e^{-\frac{1}{2}\left\{\frac{x^2}{\sigma_x^2} + \frac{(x-k)^2}{\sigma_0^2}\right\}} dx,$$

and accordingly:

$$\bar{f} = \frac{y_0\sigma_0}{\sqrt{\sigma_0^2 + \sigma_x^2}} e^{-\frac{1}{2}\frac{k^2}{\sigma_0^2+\sigma_x^2}} \quad \text{.....(xii).}$$

This is one equation linking up the indeterminate quantities y_0, σ_0, k with observable quantities like $\bar{f}$ the mean size of family in the population. We can now find the standard deviation Σ_0 of the size of families. Clearly:

$$\Sigma_0^2 = \text{Sum } (y^2)/N - \bar{f}^2,$$

$$N(\Sigma_0^2 + \bar{f}^2) = \int_{-\infty}^{+\infty} wy^2 dx$$

$$= \frac{Ny_0^2}{\sqrt{2\pi}\sigma_x} \int_{-\infty}^{+\infty} e^{-\frac{1}{2}\left(\frac{x^2}{\sigma_x^2} + \frac{(x-k)^2}{\frac{1}{2}\sigma_0^2}\right)} dx$$

$$= \frac{Ny_0^2 \frac{1}{2}\sigma_0}{\sqrt{\frac{1}{2}\sigma_0^2 + \sigma_x^2}} e^{-\frac{1}{2}\frac{k^2}{\frac{1}{2}\sigma_0^2+\sigma_x^2}}$$

as before. Hence:

$$\Sigma_0^2 = y_0^2 \left\{ \frac{\sigma_0}{\sqrt{\sigma_0^2 + 2\sigma_x^2}} e^{\frac{-k^2}{\sigma_0^2+2\sigma_x^2}} - \frac{\sigma_0^2}{\sigma_0^2 + \sigma_x^2} e^{-\frac{k^2}{\sigma_0^2+\sigma_x^2}} \right\},$$

$$V_f^2 = \left(\frac{\Sigma_0}{\bar{f}}\right)^2 = \frac{\sigma_0^2 + \sigma_x^2}{\sigma_0\sqrt{\sigma_0^2 + 2\sigma_x^2}} e^{\frac{k^2\sigma_x^2}{(\sigma_0^2+\sigma_x^2)(\sigma_0^2+2\sigma_x^2)}} - 1 \quad \text{.....(xiii).}$$

V_f is an observable quantity and this equation may be used to find k if σ_0 has been otherwise determined. The previous equation will then give y_0. We should have to go to the third moment coefficient to determine y_0, σ_0 and k from the fertility distribution, and it may be doubted whether the result would be of much value; it seems better to endeavour to determine σ_0 and k from observing the average fertilities of those exceeding certain grades of the character.

(6) Determination of the constants of the fertility distribution from the average fertilities of those possessing more than a certain grade of the character.

Let n_h be the number of the population, N, of pairs of parents who exceed the value h of the character x; then:

$$n_h = \frac{N}{\sqrt{2\pi}\,\sigma_x}\int_h^\infty e^{-\frac{1}{2}x^2/\sigma_x^2}\,dx,$$

$$n_h/N = \frac{1}{\sqrt{2\pi}}\int_{h/\sigma_x}^\infty e^{-\frac{1}{2}x'^2}\,dx'.$$

This equation determines h/σ_x from Sheppard's Tables, since n_h/N is known.

Further if $\bar{f}_h$ be the mean fertility of those over grade h:

$$n_h\bar{f}_h = \int_h^\infty \frac{N}{\sqrt{2\pi}\sigma_x}\,e^{-\frac{1}{2}x^2/\sigma_x^2}\,y\,dx,$$

$$\frac{n_h}{N}\bar{f}_h = \int_h^\infty \frac{y_0}{\sqrt{2\pi}\sigma_x}\,e^{-\frac{1}{2}\left(\frac{x^2}{\sigma_x^2}+\frac{(x-k)^2}{\sigma_0^2}\right)}\,dx$$

$$= \frac{y_0 e^{-\frac{1}{2}\frac{k^2}{\sigma_0^2+\sigma_x^2}}}{\sqrt{2\pi}\sigma_x}\int_h^\infty e^{-\frac{1}{2}\frac{\sigma_0^2+\sigma_x^2}{\sigma_x^2\sigma_0^2}\left(x-\frac{\sigma_x^2 k}{\sigma_0^2+\sigma_x^2}\right)^2}\,dx$$

$$= \bar{f}\,\frac{1}{\sqrt{2\pi}}\int_{H'}^\infty e^{-\frac{1}{2}x'^2}\,dx' \qquad \text{.....(xiv),}$$

where $$H' = \frac{\sqrt{\sigma_0^2+\sigma_x^2}}{\sigma_x\sigma_0}\left(h - \frac{\sigma_x^2 k}{\sigma_0^2+\sigma_x^2}\right).$$

Now $n_h\bar{f}_h/N\bar{f}$ is clearly known, therefore H' can be found from the tables.

We will suppose two grades of the character to be h_1 and h_2 and the corresponding average fertilities $\bar{f}_{h_1}$ and $\bar{f}_{h_2}$, then H_1' and H_2' will be known; and there results

$$H_1'\frac{\sigma_x\sigma_0}{\sqrt{\sigma_0^2+\sigma_x^2}} = h_1 - \frac{k\sigma_x^2}{\sigma_0^2+\sigma_x^2},$$

$$H_2'\frac{\sigma_x\sigma_0}{\sqrt{\sigma_0^2+\sigma_x^2}} = h_2 - \frac{k\sigma_x^2}{\sigma_0^2+\sigma_x^2},$$

$$\frac{H_1'-H_2'}{h_1-h_2} = \frac{\sqrt{\sigma_0^2+\sigma_x^2}}{\sigma_x\sigma_0}$$

Thus: $$\sigma_0 = \frac{\sigma_x}{\sqrt{\sigma_x^2\left(\frac{H_1'-H_2'}{h_1-h_2}\right)^2 - 1}} \qquad \text{.....(xv).}$$

Further: $$\frac{H_1'(h_1-h_2)}{H_1'-H_2'} - h_1 = -k\,\frac{\sigma_x^2}{\sigma_0^2+\sigma_x^2}$$

$$= -k\,\frac{\sigma_x^2(H_1'-H_2')^2-(h_1-h_2)^2}{\sigma_x^2(H_1'-H_2')^2}$$

Or, finally: $$k = \frac{H_1'h_2 - H_2'h_1}{H_1' - H_2'} \frac{1}{1 - \frac{(h_1 - h_2)^2}{\sigma_x^2 (H_1' - H_2')^2}} \quad \text{.....(xvi).}$$

Thus σ_0 and k are determined, and $\bar{f}$ being known, we can find y_0. In fact

$$y_0 = \bar{f} \frac{H_1' - H_2'}{(h_1 - h_2)/\sigma_x} e^{\frac{\frac{1}{2}\{(H_1'h_2 - H_2'h_1)/\sigma_x\}^2}{(H_1' - H_2')^2 - \frac{1}{2}\{(h_1 - h_2)/\sigma_x\}^2}} \quad \text{.....(xvii).}$$

Thus the constants of the fertility distribution are fully determined.

(7) Determination of the constants of the fertility distribution, from a knowledge of the fertility of certain grades of the population. For example, when $x = -m_1\sigma_x$, let the fertility be y_1, and when $x = m_2\sigma_x$, let the fertility be y_2. Then

$$y_1 = y_0 e^{-\frac{1}{2}\left(\frac{m_1\sigma_x + k}{\sigma_0}\right)^2},$$

$$y_2 = y_0 e^{-\frac{1}{2}\left(\frac{m_2\sigma_x - k}{\sigma_0}\right)^2}.$$

Let $\sigma_x/\sigma_0 = \lambda$, $k/\sigma_0 = \mu$; we have:

$$\sqrt{\frac{2(\log y_0 - \log y_1)}{\log e}} = m_1\lambda + \mu,$$

$$\sqrt{\frac{2(\log y_0 - \log y_2)}{\log e}} = m_2\lambda - \mu,$$

$$\left.\begin{aligned} \frac{1}{m_1 + m_2}\left\{\sqrt{\frac{2(\log y_0 - \log y_1)}{\log e}} + \sqrt{\frac{2(\log y_0 - \log y_2)}{\log e}}\right\} &= \lambda \\ \frac{1}{m_1 + m_2}\left\{m_2\sqrt{\frac{2(\log y_0 - \log y_1)}{\log e}} - m_1\sqrt{\frac{2(\log y_0 - \log y_2)}{\log e}}\right\} &= \mu \end{aligned}\right\} \text{....(xviii).}$$

The signs of the roots are arbitrary and must be selected so as to accord with the needs of the problem. For example, if $m_1 = m_2 = m$, either λm or μ may be equal to

$$\sqrt{\frac{2(\log y_0 - \log y_1)}{\log e}} \pm \sqrt{\frac{2(\log y_0 - \log y_2)}{\log e}},$$

and which we choose depends on whether the maximum fertility lies between the two grades or outside and beyond the grade of larger fertility.

The above equations assume that y_0 is known, it is the maximum average fertility of any grade in the community. A third equation to determine y_0 from the average fertility of the whole community is provided on p. 262, Equ. (xii). But the equations then become troublesome and can only be solved by approximation. It will be found also that small changes in the fertility curve are not very influential in modifying the main results to be drawn from the equations set forth in this paper.

(8) Numerical Illustrations. I propose in this section to illustrate the main results of the above investigation by selecting possible values for some of the constants. I am far from asserting that these are actually existing values, but they appear not improbable values—say, for the case of ability. We are only at present acquiring knowledge of the relationship between ability and fertility in the case of man, and the examples I give are merely illustrative and intended to indicate how the algebraical formulae can be dealt with. At the same time, I think, they throw some light on rather urgent national problems. It is desirable to bring home to the minds of the thinking classes what it really does mean, if the fittest in any character have not a third of the fertility of the least fit.

Illustration I. The upper decile of a population has an average fertility rate of 2, the 50 °/₀ showing the lower values of the character an average fertility rate of 6 The fertility of the whole population has a mean value of 5. Find the changes in the population during one generation.

Probability integral tables give at once $h_1 = 1{\cdot}2816\sigma_x$.

To determine H_1', we have

$$n_{h_1} \times \bar{f}_{h_1}/N \times \bar{f} = {\cdot}1 \times 2/5 = {\cdot}04,$$

therefore

$$H_1' = 1{\cdot}7500.$$

Clearly $h_2' = 0$, $n_{h_2}\bar{f}_{h_2}/N \times \bar{f} = {\cdot}5 \times 6/5 = {\cdot}6$, from which the tables give us $H_2' = {\cdot}2533$.

A rough diagram indicates at once the sign of the quantities and shows that we must have

$$H_1' = h_1 + \sigma_x{}^2 k/(\sigma_x{}^2 + \sigma_0{}^2)$$
$$H_2' = \sigma_x{}^2 k/(\sigma_x{}^2 + \sigma_0{}^2) - h_2$$

or in this case

$$1{\cdot}7500\sigma_x\sigma_0/\sqrt{\sigma_0{}^2 + \sigma_x{}^2} = 1{\cdot}2816\sigma_x + \sigma_x{}^2 k/(\sigma_0{}^2 + \sigma_x{}^2),$$
$${\cdot}2533\sigma_x\sigma_0/\sqrt{\sigma_0{}^2 + \sigma_x{}^2} = \sigma_x{}^2 k/(\sigma_0{}^2 + \sigma_x{}^2);$$

or, subtracting:

$$1{\cdot}4967\sigma_x\sigma_0/\sqrt{\sigma_0{}^2 + \sigma_x{}^2} = 1{\cdot}2816\sigma_x,$$

whence

$$\sigma_0 = 1{\cdot}6579\sigma_x,$$

and on substituting

$$k = {\cdot}8131\sigma_x.$$

The equation

$$\bar{f} = \frac{y_0\sigma_0}{\sqrt{\sigma_0{}^2 + \sigma_x{}^2}}\, e^{-\frac{1}{2}\frac{k^2}{\sigma_0{}^2 + \sigma_x{}^2}}$$

will now give us y_0. On substituting the values of σ_0 and k and 5 for $\bar{f}$ we find

$$y_0 = 6{\cdot}439.$$

The distribution of fertility is accordingly given by

$$y = 6{\cdot}439e^{\frac{1}{2}\left(\frac{x - {\cdot}8131\sigma_x}{1{\cdot}6579\sigma_x}\right)^2}$$

It is clear therefore that the fertility rises to a maximum at $\cdot 8131\sigma_x$ and then falls again, so that the lowest values of the character have a lowered fertility, but are still in excess of the corresponding high values. Thus m_1, the mean of the effective parentages, is given by

$$m_1 = k\sigma_x^2/(\sigma_0^2 + \sigma_x^2) = \cdot 2169\sigma_x,$$

measured *below* the old mean, and

$$\Sigma_1 = \text{standard deviation of effective parentages}$$
$$= \sigma_0\sigma_x/\sqrt{\sigma_x^2 + \sigma_0^2} = \cdot 8563\sigma_x,$$

or, the effective parentages have 16 per cent. less variability than individual parentages, i.e. parentages unweighted with their offspring. We can now find the average fertility of the lowest decile of the population:

$$\frac{n_{-h_1}\bar{f}_{-h_1}}{N\bar{f}} = \frac{1}{\sqrt{2\pi}}\int_q^\infty \frac{1}{\Sigma_1} e^{-\frac{1}{2}\left(\frac{x''}{\Sigma_1}\right)^2} dx''$$
$$= \frac{1}{\sqrt{2\pi}}\int_{q'}^\infty e^{-\frac{1}{2}x'^2} dx',$$

where
$$q' = q/\Sigma_1 = \frac{h_1 - m_1}{\Sigma_1}$$
$$= \frac{1\cdot 2816 - \cdot 2169}{\cdot 8563} = 1\cdot 2434.$$

Hence
$$\frac{n_{-h_1}\bar{f}_{-h_1}}{N\bar{f}} = \frac{1}{10}\frac{\bar{f}_{-h_1}}{5} = \cdot 10686,$$

or,
$$\bar{f}_{-h_1} = 5\cdot 34;$$

that is to say, that while the average value of the uppermost decile's fertility is only 2, that of the lowermost decile is 5·34, i.e. is still in excess of the average fertility, 5, of the population. Indeed for only about 6 per cent. of the population with the very lowest values of the character does the fertility fall below some 4 per cent.

If we take N as before for the number of pairs of parents, then the distribution of effective parentages is given by

$$w = \frac{5N}{\sqrt{2\pi}}\frac{1}{\cdot 8563\sigma_x} e^{-\frac{1}{2}\left(\frac{x - \cdot 2169\sigma_x}{\cdot 8563\sigma_x}\right)^2}.$$

We have now to consider the distribution of offspring from these effective parentages. We have to consider ρ of p. 260 in relation to σ_x and σ_z. Four hypotheses are possible:

(i) We may suppose that the character is equally influential in the case of the fertility of both sexes; thus brain capacity and resulting intellectual energy might mark equal reduction of fertility in both sexes. In this case the proper value for x is*

$$x = \sigma_1\left(\frac{\xi_1}{\sigma_1} + \frac{\xi_2}{\sigma_2}\right).$$

* Here as later it must be borne in mind that a constant factor is of no importance.

This gives at once

$$\sigma_x = \sigma_1 \sqrt{2} \sqrt{1 + r_{12}},$$

$$\rho_{xz} = \frac{1}{\sqrt{2}} (r_{1z} + r_{2z}) / \sqrt{1 + r_{12}}.$$

Here if the population were stable we should have $\sigma_1 = \sigma_z$, 1 and 2 representing as before mother and father. According to our present knowledge $r_{1z} = r_{2z} = \cdot 4$ to $\cdot 5$, = say, $\cdot 45$, and r_{12} lies between $\cdot 15$ and $\cdot 25$, say, $\cdot 2$. We have then

$$\sigma_x = \sigma_z \times 1 \cdot 5492$$

and ρ_{xz}, $= \rho$ of our formulae on pp. 260—1, $= \cdot 5809$.

(ii) We may suppose that the character is equally influential with the fertility of both sexes, but that there is no assortative mating. In this case $r_{12} = 0$, the coefficients of $\frac{\xi_1}{\sigma_1}$ and $\frac{\xi_2}{\sigma_2}$ must still be equal and we have at once

$$\sigma_x = \sigma_z \times 1 \cdot 4142, \quad \rho = \cdot 6364.$$

(iii) It is conceivable that the character affects the fertility in one sex only and that there is no assortative mating. In this case we may take $x = \xi_1$, and if the population is stable $\sigma_x = \sigma_z$, while $\rho = \cdot 45$.

(iv) The character may be supposed to affect one sex only, but there may be assortative mating. In this case, returning to our formula on p. 259, $r_{23} = 0$ and x may be taken proportional to

$$\frac{r_{13}}{1 - r_{12}^2} \frac{\xi_1}{\sigma_1} - \frac{r_{12} r_{13}}{1 - r_{12}^2} \frac{\xi_2}{\sigma_2},$$

or we may write

$$x = \sigma_1 \left(\frac{\xi_1}{\sigma_1} - r_{12} \frac{\xi_2}{\sigma_2} \right),$$

which leads to

$$\sigma_x = \sigma_1 \sqrt{1 - r_{12}^2}, \quad \text{and} \quad \rho_{xz} = \frac{r_{1z} - r_{12} r_{2z}}{\sqrt{1 - r_{12}^2}},$$

or on the same assumptions as before:

$$\sigma_x = \sigma_z \times \cdot 9798, \quad \text{and} \quad \rho = \cdot 3674.$$

In all these cases σ_z and ρ are of course the variability of offspring for the given character and their correlation with the parentage character on the assumption of one child to the parentage. We now proceed to discuss the constants of the offspring distribution as resulting from effective parentages, i.e. the quantities m_2, Σ_2 and R of our p. 261. Here m_2 is the reduced mean value of the character,

Constants of Offspring Distribution. Illustration I.

Hypothesis ...	(i)	(ii)	(iii)	(iv)
m_2/σ_z ...	·1260	·1380	·0976	·0797
Σ_2/σ_z ...	·9539	·9445	·9747	·9818
R/ρ ...	·8976	·9065	·8805	·8721

i.e. the regression owing to differential fertility, Σ_2 is the variability of the offspring generation and should be compared with σ_z or σ_1, while R is the correlation between offspring and effective parentages.

An examination of this table shows the following general results: (*a*) the mean value of the character in the population is reduced 8 to 13 per cent. of its standard deviation according to the hypothesis we make with regard to the influence of the character on fertility. That is to say, the mean character of the population stands one-third to a half-decile lower with each new generation. (*b*) In the next place the variability is lowered two to five per cent. The probability therefore of extreme deviations is sensibly reduced; but this reduction tells only on excess of the character because the mean of the distribution has been markedly shifted towards defect of the character; in other words, if extremes of the character in excess correspond to noteworthy men, they are much less likely to occur in a population where this differentiation of fertility has set in. Suppose we term one man in a thousand "noteworthy," then such a man corresponds to the grade $3{\cdot}090\sigma_1$ in our original population. His position in the offspring generation will be

$$m_2 + 3{\cdot}090\sigma_1 = 3{\cdot}216\sigma_1 = \frac{3{\cdot}216}{{\cdot}9539}\Sigma_2 = 3{\cdot}37\Sigma_2.$$

But this corresponds to four such individuals in 10,000, or 1 in 2500; or there will be less than half as many noteworthy men. This is on the first hypothesis; the second hypothesis shows only 1 in 3300, and the fourth or less influential hypothesis 1 in 1700. It will therefore be obvious that if mental powers are hereditary, any differentiation of fertility will not only alter rapidly the average intelligence of the community, but will produce that dearth of noteworthy ability, or of genius, which is fatal to the continued existence under modern competitive conditions of a great national life. The whole system of curves corresponding to this first Illustration is given in Diagram I.

Illustration II. In the previous illustration we considered a case where the maximum fertility did not coincide with the minimum of the character, although the lowest values of the character had markedly higher fertility than the highest. It will be of interest to determine what, if any, differences arise in our general conclusions, if we fix on the fertility of individuals high and low in the scale, instead of on average fertility values. We will suppose that the individual parentage which stands sixth in the 1000 for the given character has only a fertility of 1·5, and that which stands last but six has a fertility of 6·5; the maximum fertility of any grade shall be 7, and this shall occur *below the grade* of the individual parentage with a fertility of 6·5. Using the notation of p. 264, we may take $m_1 = m_2 = 2{\cdot}5$, say. Hence

$$\pm\sqrt{\frac{2\,(\log 7 - \log 1{\cdot}5)}{{\cdot}434{,}2945}} = 2{\cdot}5\lambda + \mu = \pm\,1{\cdot}75525,$$

$$\pm\sqrt{\frac{2\,(\log 7 - \log 6{\cdot}5)}{{\cdot}434{,}2945}} = 2{\cdot}5\lambda - \mu = \pm\,{\cdot}38503.$$

Since λ must be positive, these give us

$$2{\cdot}5\lambda = {\cdot}6851, \quad \mu = 1{\cdot}0701, \quad \text{or} \quad 2{\cdot}5\lambda = 1{\cdot}0701, \quad \mu = {\cdot}6851.$$

In order that the maximum fertility may be outside the range $-2{\cdot}5\sigma_x$ to $2{\cdot}5\sigma_x$, we must take the first pair of roots. We easily find

$$\sigma_0 = 3{\cdot}6490\sigma_x, \quad k = 3{\cdot}9049\sigma_x,$$

and from equation (xii) $\quad \bar{f} = 4{\cdot}2613.$

Hence the fertility distribution is

$$y = 7e^{-\frac{1}{2}\left(\frac{x - 3{\cdot}9049\sigma_x}{3{\cdot}6490\sigma_x}\right)^2}.$$

The maximum fertility 7 occurs so far from the mean value of the character, that there are practically no parentages with a low value of the character and a low fertility, i.e. the number of *individual* parentages with an extremely low value of the character and a fertility under 7 is only 1 in the 20,000. We may therefore look upon our curve as one which sensibly describes a fertility constantly decreasing as the value of the character increases.

Turning to the distribution of effective parentages we find:

$$m_1 = {\cdot}2728\sigma_x, \quad \Sigma_1 = {\cdot}9644\sigma_x.$$

Thus there is rather a greater shift than in Illustration I. of the average effective parentage, but the parentages are more variable. The distribution of effective parentages is:

$$w = \frac{4{\cdot}261N}{\sqrt{2\pi} \times {\cdot}9644\sigma_x} e^{-\frac{1}{2}\left(\frac{x - {\cdot}2728\sigma_x}{{\cdot}9644\sigma_x}\right)^2}.$$

The number of *effective* parentages with low values of the character and fertility less than the maximum will be found from determining the area of this w curve for values of x greater than $x = 3{\cdot}9049\sigma_x - {\cdot}2728\sigma_x = 3{\cdot}6321\sigma_x$, or by looking up in the Tables $x' = 3{\cdot}6321/{\cdot}9644 = 3{\cdot}77$; this gives 1 in 12,500, a negligible frequency.

In view of our previous results for Illustration I, it may be of interest to note the average fertilities of the top and bottom deciles of our individual parentages, they are to be found from:

$$\frac{n_h \bar{f}_h}{N\bar{f}} = \frac{1}{\sqrt{2\pi}} \int_{q'}^{\infty} e^{-\frac{1}{2}x'^2} dx',$$

where $$q' = \frac{h - m_1}{\Sigma_1} = \frac{\pm 1{\cdot}2816 - {\cdot}2728}{{\cdot}9644},$$

whence by aid of the tables we find 2·28 for the average fertility of the top decile of the individual parentages, and 6·30 for the bottom decile. These values may be compared with the corresponding 2 and 5·34 of Illustration I.

We now turn to the distribution of offspring. We have:

$$m_2 = \cdot 2728\rho\sigma_z, \quad \Sigma_2 = \sigma_z\sqrt{1 - \cdot 06986\rho^2},$$

$$R/\rho = 3\cdot 6490/\sqrt{14\cdot 3152 - \rho^2}.$$

We can now determine these in precisely the same manner as they were tabled for the four hypotheses in Illustration I.

Constants of Offspring Distribution. Illustration II.

Hypothesis ...	(i)	(ii)	(iii)	(iv)
m_2/σ_z ...	·1585	·1736	·1228	·1002
Σ_2/σ_z ...	·9881	·9858	·9929	·9953
R/ρ ...	·9759	·9783	·9713	·9689

We see at once from this table that whichever hypothesis we take the variability of the new generation is only lessened about 1 per cent. and the correlation about 3 per cent., values markedly less than in the case of Illustration I. This is obviously due to the fact that the fertility distribution is markedly less concentrated. But the shift of the population average is now 10 to 17 per cent. of the standard deviation, or roughly the average has receded through $\frac{1}{2}$ to $\frac{2}{3}$ of a decile in the character. We may measure the extreme effect of this by inquiring how far "noteworthy" individuals, the individual one in a thousand of the old generation, have been reduced in number. What we have to find is the value of $(m_2 + 3\cdot 090\sigma_1)/\Sigma_2$; for the first hypothesis this is 3·288, which corresponds to 1 in 2000, or the halving of these noteworthy individuals in the second generation, and we have practically the same rate on the second hypothesis. We thus see that on the numbers of this second Illustration, the average population will degenerate considerably faster, 25 per cent. faster approximately, but the dearth of noteworthy individuals will be slightly less. The reader must not, however, conclude from these numbers that our second Illustration represents a less undesirable type of degeneracy. Beside the dearth of the noteworthy, the multiplication of the extremely unfit has to be measured. Let us suppose that the individual whose character has the least value in the 1000 of the original population is by virtue of this deviation a non-useful member of the community—physically or mentally unfit. We must clearly then find out in the second generation to what number of the population this standard applies, i.e. we want $(3\cdot 090\sigma_1 - m_2)/\Sigma_2$. This is equal to 2·967 on the first and 2·958 on the second hypothesis, corresponding in both cases to 1 in 667 instead of 1 in 1000. On the other hand it will be found that in the first illustration the corresponding numbers are 3·107 and 3·125 on hypotheses (i) and (ii) respectively, giving practically 1 in 1000, and the same standard of unfitness repeating itself in the second generation. Thus Illustration II, with a slightly reduced rate of decrease of noteworthy individuals, has an increased rate of unfit individuals, when compared with

Illustration I, and a general or average decadence greater by 25 per cent. It would be idle to assert that one or other corresponds to a greater racial degeneracy until we have settled whether it is more important to a nation to have in the case of a desirable character (i) a high average value, (ii) a larger percentage of noteworthy individuals, or (iii) a smaller percentage of unfit individuals. We do not know at present what relative weight is to be given to these categories; but we can see that in any quantitative theory of the socially desirable characters in a community, they naturally arise from the analysis of a differential fertility. Such factors as dearth of leaders, lowered general intelligence of the community, multiplication of the unfit, are seen, even by a preliminary analysis of this kind, not to be unrelated causes, by aid of which the historian accounts in a vague manner for racial and political changes. They are associated factors, explicable and measurable when we know the outlines of a theory of differential fertility.

No special stress must be laid on the particular numbers chosen above to illustrate this discussion. They are round numbers not wholly inappropriate, perhaps, to what we know of intelligence and fertility in man. They are used to suggest the main outlines of a discussion of such problems. Considering the immense masses of statistical, especially demographic data now being accumulated in all civilised countries, it seems to me that the historian of the future will have the means of testing in a way, never yet feasible, the changing fitness or unfitness of nations. In that future absolute and differential fertility, the abundance or dearth of leaders, the average intelligence of a nation, the reduction or multiplication of its unfit, will be recognised as the basal factors in racial progress, the biological sources of evolution in political history. Then history becoming biological will for the first time be admitted as a branch of scientific inquiry, and will also for the first time provide the requisite training for the statesman.

If we turn to examine the possibilities of immediate work with regard to differential fertility, there are several directions in which investigations can at present be carried out with reasonable hope of success. If we confine ourselves to the artizan class, wages are a very reasonable measure of capacity, and the correlation between these and fertility or fecundity* might fairly easily be ascertained. Intelligence might be measured by craft classification extending from engineer to general labourer. Much may be done, if we pass from the artizan class alone, by comparing the size of families of members of various professions, of those following mercantile pursuits, of retail traders and of artizans. Further within a narrow class like the graduates of the Universities, it ought to be feasible actually to tabulate fertility against a scale of intelligence as measured by academic distinction. Fertility of sane and insane stocks, of mentally defective and normal stocks, of alcoholic and sober parentages can be ascertained, although the task, of course, needs careful social inquirers. Lastly grades of physical fitness might well be investi-

* Fertility might be measured by nett or gross family when complete, or for marriages of 15 or 20 years' duration. It would, I think, be possible to measure fecundity by some function of the intervals between the births of the first three or four children.

gated; the family of the athletic might be compared with that of the non-athletic; and those of the smith and the navvy with those of the cobbler, tailor, and street hawker. It would, I think, be quite possible even without a reformed census, for which every sociologist should work, to obtain many results of value bearing on differential fertility by inquiries of the above nature.

Diagrams.

Each diagram consists of five normal curves. They are worked out on Hypothesis (i), i.e. that the fertility of the parentage is equally affected by the character in both parents, and that there is an assortative mating in the community. The coefficient of parental heredity is taken as ·45 and that of assortative mating as ·20. x is measured from the mean of the character in the parentage of the first generation. Expressing everything in terms of σ_1 the variability, say, of males in the first generation we have the following curves:

Diagram I. Representing Illustration I.

Curve I. Distribution of Males in first generation:

$$y = \frac{n}{\sqrt{2\pi}\,\sigma_1} e^{-\frac{1}{2}\frac{x^2}{\sigma_1^2}}.$$

Curve II. Distribution of Individual Parentages in first generation:

$$y = \frac{n}{\sqrt{2\pi}\,1{\cdot}55\sigma_1} e^{-\frac{1}{2}\frac{x^2}{(1{\cdot}55\sigma_1)^2}}.$$

Curve III. Curve of Fertility of Individual Parentages of first generation:

$$y = 6{\cdot}439\, e^{-\frac{1}{2}\left(\frac{x - 1{\cdot}2603\sigma_1}{2{\cdot}5697\sigma_1}\right)^2}.$$

(N.B. Only part of this curve is drawn.)

Curve IV. Distribution of Effective Parentages in first generation:

$$y = \frac{5n}{\sqrt{2\pi}\,1{\cdot}3273\sigma_1} e^{-\frac{1}{2}\left(\frac{x - {\cdot}3362\sigma_1}{1{\cdot}3273\sigma_1}\right)^2}.$$

Curve V. Distribution of Males in second generation:

$$y = \frac{5n}{\sqrt{2\pi}\,{\cdot}9539\sigma_1} e^{-\frac{1}{2}\left(\frac{x - {\cdot}1260\sigma_1}{{\cdot}9539\sigma_1}\right)^2}.$$

Diagram II. Representing Illustration II.

Curve I. Distribution of Males in first generation:

$$y = \frac{n}{\sqrt{2\pi}\,\sigma_1} e^{-\frac{1}{2}\frac{x^2}{\sigma_1^2}}.$$

Curve II. Distribution of Individual Parentages in first generation:

$$y = \frac{n}{\sqrt{2\pi}\,1{\cdot}55\sigma_1} e^{-\frac{1}{2}\frac{x^2}{(1{\cdot}55\sigma_1)^2}}.$$

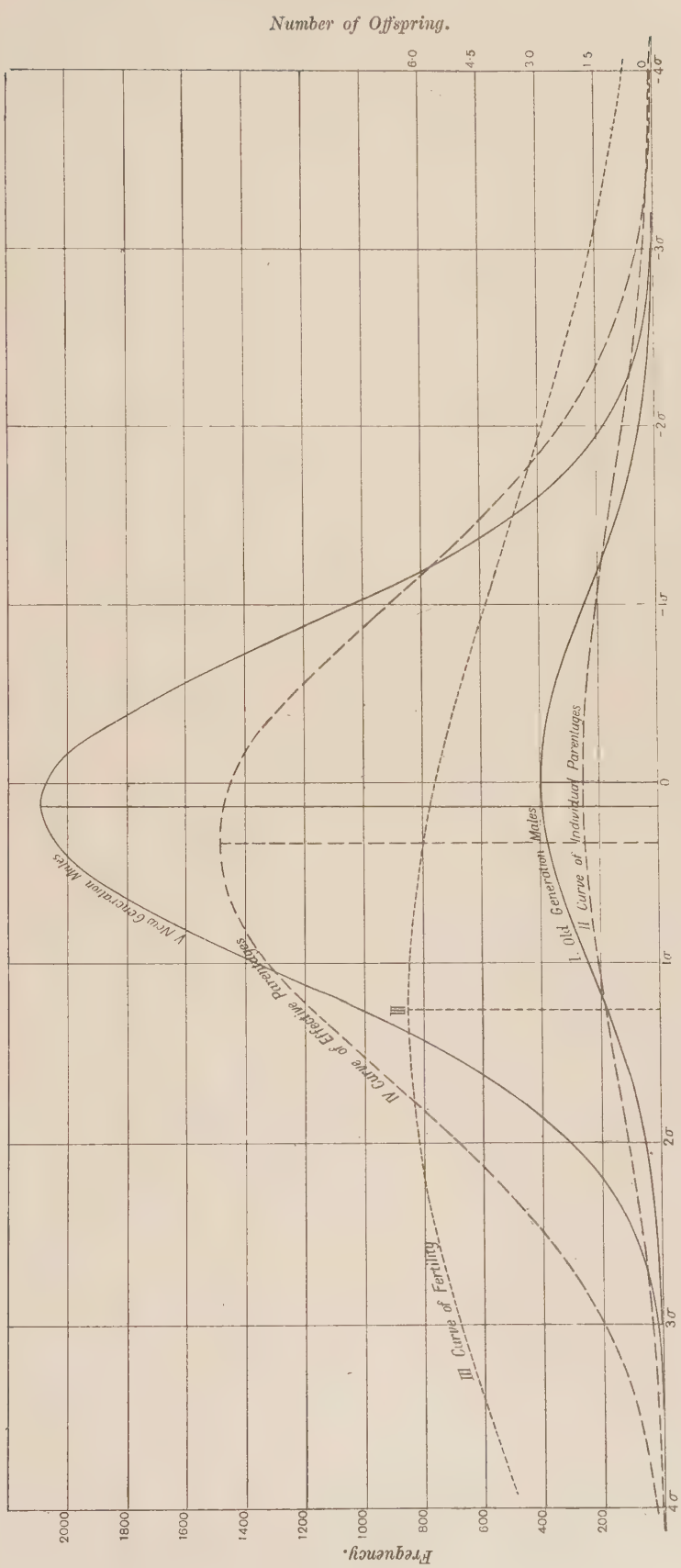


DIAGRAM I. Fertility has its maximum well within range of parental variation.

In this diagram the value of the character is supposed to *increase* from left to right, but x is positive when it is a *defect* from the mean value.

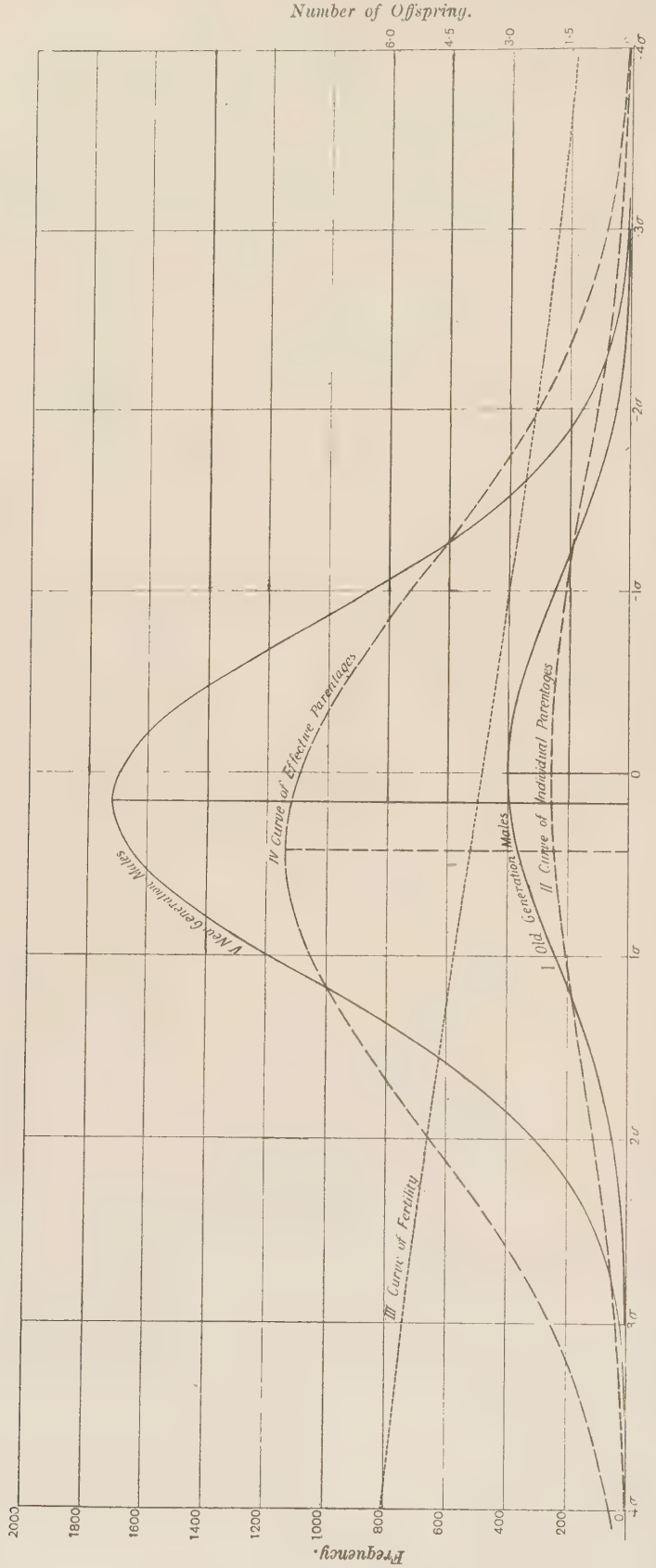


DIAGRAM II. Fertility has an almost uniform gradient within the range of parental variation, its maximum lying practically outside that range.

In this diagram the value of the character is supposed to *increase* from left to right, but x is positive when it is a *defect* from the mean value.

Curve III. Curve of Fertility of Individual Parentages of first generation:

$$y = 7e^{-\frac{1}{2}\left(\frac{x - 6{\cdot}0526\sigma_1}{5{\cdot}6560\sigma_1}\right)^2}.$$

(N.B. Only one half of this curve is drawn.)

Curve IV. Distribution of Effective Parentages in first generation:

$$y = \frac{4{\cdot}261n}{\sqrt{2\pi}\,1{\cdot}4948\sigma_1} e^{-\frac{1}{2}\left(\frac{x - {\cdot}4228\sigma_1}{1{\cdot}4948\sigma_1}\right)^2}.$$

Curve V. Distribution of Males in second generation:

$$y = \frac{4{\cdot}261n}{\sqrt{2\pi}\,{\cdot}9881\sigma_1} e^{-\frac{1}{2}\left(\frac{x - {\cdot}1585\sigma_1}{{\cdot}9881\sigma_1}\right)^2}.$$

A STATISTICAL STUDY IN CANCER DEATH-RATES.

By G. D. MAYNARD, F.R.C.S.E., Pretoria.

As there is still much that remains obscure in the aetiology of Cancer, any new light that can be thrown on this important subject seems worthy of careful consideration.

It is with a sense of great diffidence that I bring these figures forward, knowing full well how liable to error death-rates may be, and how easy it is to draw false conclusions from the study of figures. But when all is said and done it is to figures that the final appeal must be made, and if the death-returns are so unreliable as to be worthless, then it is certain that there are no more reliable data to be obtained. The comparatively few cases that come within any individual experience are, from the point of view of exact knowledge, of little or no value.

If therefore we are to learn anything from the history of the past, it is to figures that we must go, guardedly and with great caution, bringing all the collateral evidence that we can obtain, to check and control our results, and above all using accurate methods of analysis before drawing any conclusions.

Theories are frequently being advanced, in medical literature, in conflict with the records, these either being ignored, or set aside with the remark that they are so unreliable that no attention should be paid to them. Marked divergencies in the rates are dismissed as being due to errors in registration, the increasing cancer death-rate is regarded as being due to better diagnosis, and so on.

Before entering on any discussion as to the causes of the varying cancer death-rates as observed in different countries—and even in the cities and towns of the same country—it will be advisable to make a few general remarks on how the data in this paper have been obtained and dealt with, and to answer a few of the objections that may be raised as to the value of the returns themselves.

At the outset one must admit that many imperfections are inherent in all statistics of death which are not based on post-mortem examinations. But because the figures are not absolutely correct are we to neglect them, or regard them as absolutely useless? One must remember that where large masses of figures are being dealt with the errors in excess tend to balance those in defect, and wrong

inclusions in a group must be set against the accidental exclusions. For instance, is it reasonable to consider that in two cities of the same country, during the same period, and under similar Registration Laws, rates of 1184 and 446 per million are merely fortuitous or due to errors of registration? Such a suggestion means that either more than half the cases are overlooked in the one town or more than 100% too many are recorded in the other, unless the error is to be divided between them. Before dismissing all differences in rates as being due to errors in diagnosis or imperfections of record it is well to realise exactly what this suggestion entails.

Not only is it often said that better registration accounts largely for the varying rates in different places, but that much of the increase in the cancer death-rate, as also that occurring in the diabetes death-rate, is due to better diagnosis. The statistics show that both these diseases are very markedly on the increase. As a rule before death occurs both diseases give a very definite clinical picture, and run a more or less protracted course. When dealing with death returns, difficulties in early diagnosis are of no consequence. These factors are in favour of a correct diagnosis being made, and probably fewer mistakes are made in the diagnosis of these groups than in almost any others. In the last 30 years there has been very little added to our knowledge, that would aid the general practitioner to a more accurate diagnosis, in the final stages of these diseases. Doubtful tumours are more often submitted to the microscope for confirmation of diagnosis than formerly, but the effect of this on the returns would rather be to exclude from the cancer group cases that would otherwise have been found there; for a fatal tumour of doubtful origin would most probably have been classed as cancer in the absence of definite knowledge to the contrary. There does not seem therefore, sufficient ground for believing that the increase in the rates is in any great part due to improved medical knowledge, or that this will account for the growth in the cancer and diabetes death-rates.

If we refer to the cancer death-rates for "occupation groups" we find that those consisting of men who by their social status can best afford highly skilled medical care, or those who have State medical attendance provided, are the groups showing the lowest death-rates, while the labouring and agricultural classes show distinctly higher rates. This is the reverse of what one would be led to expect if the explanation that better diagnosis accounts for the increased rate—is the correct one.

There is still a further reason for thinking that the group returned as cancer is fairly homogeneous in character. It will be seen from Fig. 1, how good a fit a Pearson Curve gives for the male death-rates, tabulated in five-yearly periods. The dotted polygons at ages 92·5 and 97·5 are the original observations and the constants of this curve are given below. Sheppard's adjustments were not used as I did not expect to find high contact at both ends. Finding however that there was high contact I recalculated the curve, using Sheppard's adjustments and

altering the two groups referred to above, as indicated in the diagram. As these groups are based on very small numbers, this step seemed justifiable. The general fit is slightly improved thereby*, although the form of the curve is not altered. In Fig. 2, the female curve is given. Here the observations are more irregular, which may possibly be due to the greater difficulty in obtaining correct ages in the

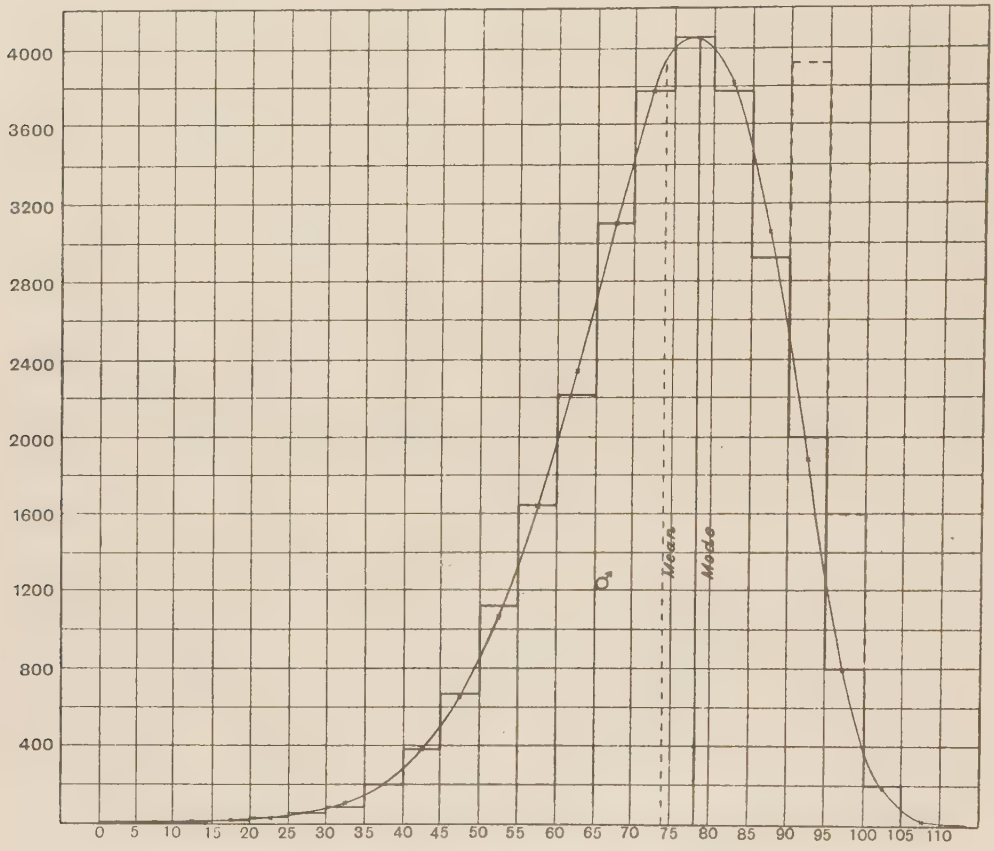


Fig. 1. Cancer Death-Rate, Males, U.S.A., 1900.

$$y = y_0 \left(1 + \frac{x}{52{\cdot}3713}\right)^{45{\cdot}6} \left(1 - \frac{x}{7{\cdot}0852}\right)^{6{\cdot}17}.$$

case of women. Sheppard's adjustments were used, although the result shows that we are not justified in supposing high contact. The total number of cases employed in finding the rates for the various age periods was male deaths 11,436, female deaths 18,039. The areas of the frequency polygons represent the number of cancer deaths per million of population living at each age.

* That the theoretical start of the modified curve falls so long before birth is, perhaps, due to the fact that there is really not high contact at the old age end after modification.

The constants of the curves are:—

	Male, Fig. 1		Female, Fig. 2
	Unaltered	Altered	
Mean ...	75·527375	73·906005	72·220620
Mode ...	78·764200	78·116220	78·681705
S. D. ...	2·969692	2·730771	3·048332
μ_3 ...	−7·422077	−11·657840	−14·515937
$\sqrt{\beta_1}$ ...	·283393	·572483	·512457
β_2 ...	2·628036	3·377277	2·818529
κ_1 ...	− ·984864	− ·228657	− 1·150781
Start ...	14·972165	−83·740300	2·406680
End ...	112·200885	113·542075	102·542040

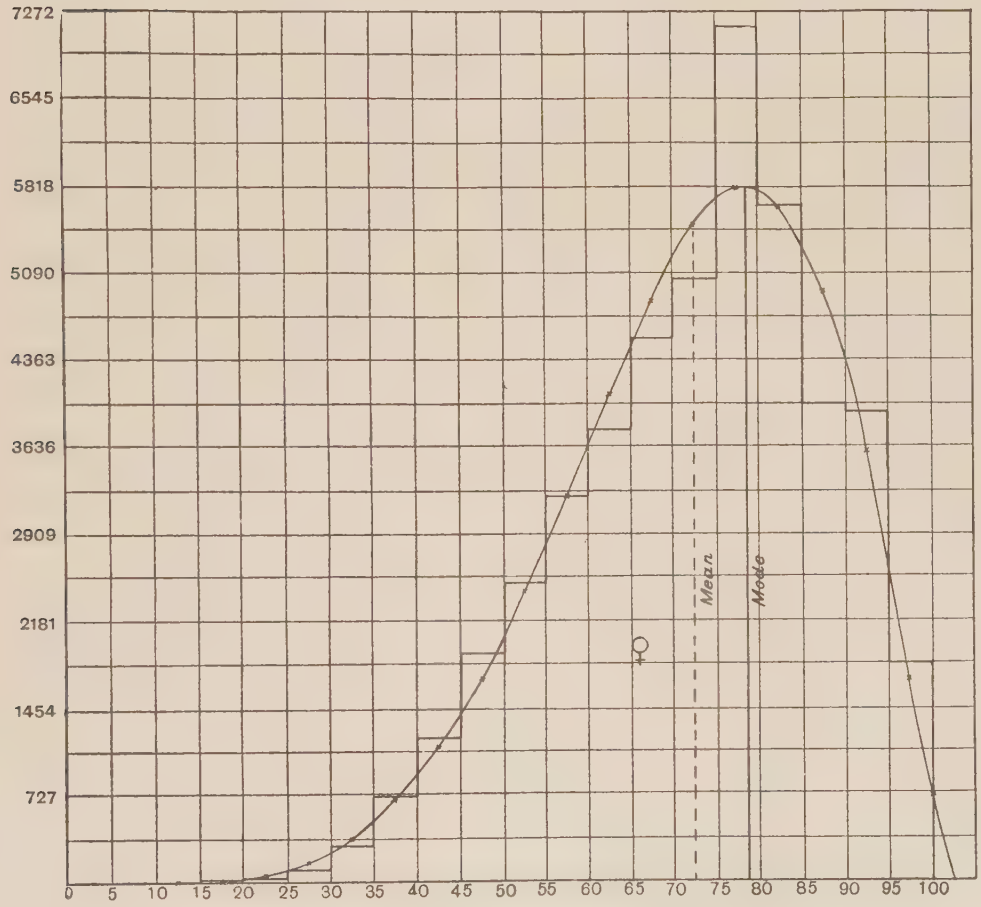


FIG. 2. Cancer Death-Rate, Females, U.S.A., 1900.

$$y = y_0 \left(1 + \frac{x}{15{\cdot}2550}\right)^{4{\cdot}656} \left(1 - \frac{x}{4{\cdot}7721}\right)^{1{\cdot}456}.$$

Readers who have fitted curves to "disease" distributions grouped in this manner will I think agree, that a fit as good as that given in Fig. 1 indicates the homogeneous nature of this group. I have frequently been struck with the excellent fit given by diseases, like puerperal septicaemia, where the chance of a wrong diagnosis is unlikely, and greatly disappointed with the bad fit obtained by a disease that presents difficulty in diagnosis, such for instance as enteric fever in hot climates, where the heterogeneous character of the group is undoubted. The excellence of fit in the case of the male cancer curve is I think evidence of some value as to the homogeneity of these returns.

The goodness of fit as measured by the usual χ^2 formula* for the modified male curve is $P = \cdot 4$ and excluding the irregular groups at 2·5 and 107·5, $P = \cdot 73$.

The curve in the case of the diabetes distribution is not satisfactory. No single curve can be fitted to the figures which will even approximately represent the frequency polygon. It seemed advisable to see if two or more curves could be found that would serve to express the recorded rates. It is fairly easy to see that these curves cannot all be normal. The old age component of diabetes is clearly skew, and it was not hard to obtain a good representation of this part of the mortality. The earlier mortality appears to consist of two components; a diabetes of youth with a mean age of 15 to 20, and a diabetes of middle age with a mean age of about 40. These components would agree to some extent with those found by Pearson in his resolution of the general mortality curve†. The discussion of this point has been reserved for another paper. Further figures are needful before it can be definitely stated that the disease now known as *Diabetes melitus* can be certainly resolved into two or more age groups. The English death returns for this disease show the same general features as those obtained from the United States‡.

Before any comparisons of the cancer death-rates can be profitably made it is necessary that they should be corrected for the age constitution of the population. Cancer is mainly a disease of advanced life, and as this is the portion of the population most unevenly distributed between town and country, the difference in crude death-rates due to this cause must be borne in mind when rates are being compared. Similarly no useful comparisons can be made between the rates as found for the different occupations until corrected for age. The group of bankers for instance has a very different age composition from that of clerks, as the following correction factors show: bankers = 0·3516: clerks = 1·3157. Thus the crude rates of 414 and 280 become with correction 150 and 368 respectively.

The usual method§ of obtaining the correction factor has been adopted, and

* *Biometrika*, Vol. I. p. 155. † *Phil. Trans.* Vol. 186, p. 407.

‡ The female diabetes frequency polygon exhibits the same features as the male distribution, the rates being similar. The early age portions of both cancer and diabetes frequencies require special consideration, which I hope to give on another occasion.

§ R = death-rate in standard population, r_1, $r_2 \ldots r_n$ = death-rates at age periods, x_1, $x_2 \ldots x_n$ = numbers of population in similar age groups, for district to be corrected, where X is total population. Then if $R' = S(xr)/X$, R/R' is the correction factor.

the method by which the standard death-rate used was obtained is given in the Supplement to the 65*th Annual Report* of the Registrar-General, for the years 1891—1900. The standard population is that of England and Wales for the same period. It is immaterial what standard is adopted so long as all crude rates are reduced to the same base, as they can then be compared *inter se,* although they do not represent the actual rate obtaining in the district considered.

The only source at my disposal from which it was possible to obtain the necessary data to correct the rates for towns and districts, was the United States Census of 1900. Fortunately this country covers a large area, and is subject to many varying influences of climate and social conditions. It is thus a particularly suitable country in which to study the conditions which may predispose to the development of this disease. Unfortunately, however, the whole country is not under a uniform registration law; the field is therefore restricted to those states and cities which are. The statistics from these Registration States and Cities are probably fairly accurate; and I think the marked differences in rates that are observed, cannot be due to faults of registration, and point to real differences in predisposing causes. As a check on the accuracy of the death returns the Census Department made an enumeration of the deaths occurring in the year ending May 31st, 1900. This was carried out in all districts where there was any doubt as to the state of the registration. The results show that so far as the cities dealt with in this paper are concerned, the error in no case exceeds 9·5 %. In only one city was the error greater than 6·5 %, while the mean error is 3·5 % with a coefficient of variation of 2·225 %.

In addition to the correction for age, a sex correction might also have been introduced. In towns of over 100,000 inhabitants—and none with a smaller population than this have been used in this paper—the variation in sex distribution is not, from our present point of view, of very great importance, while the extra arithmetical labour involved in calculating over 150 further correction factors is very considerable. In a few cases where the sex distribution seemed to vary considerably from the usual proportions, I calculated the sex correction factor, but in no case did the alteration in the corrected rate amount to 0·5 %. It should be remembered that the cancer death-rate is highest for females, while the diabetes death-rate is highest for males, thus any error resulting from this omission would tend to lower the correlation that exists between these diseases.

In dealing with large areas of mixed urban and rural populations another source of disturbance must be borne in mind, viz. the different age distributions in the two districts, with its effect on the death-rates in each. The correction factor for the rural population of the ten Registration States in 1900 is for cancer ·7585, while for the urban areas of the same States it is 1·0617. Now as the proportion of the population in the urban and rural districts differs in each State, this in itself will cause an appreciable difference in the observed rates. The following table shows the change in rate that would be made from this cause alone, were the age distribution of rural and urban districts uniform for each State.

Cancer Death-Rates.

State	Crude	Corrected
Maine	837	692
New Hampshire ...	668	568
Vermont	818	647
Massachusetts ...	664	648
Rhode Island ...	623	590
New York	612	587
Connecticut	611	563
New Jersey	504	472
Michigan	580	498

It will be noted that this correction although lowering all the rates does not materially disturb their order.

The correction factor as used for the various States is based only on the age distribution of the whole State without reference to the proportions in the urban and rural areas. The necessary figures to make this latter correction were unobtainable.

Throughout the paper the correction factors are based on the census of 1900 while the death-rates of the cities are those of the five-year group 1900—1904; it being assumed that the age distribution has kept fairly uniform, or at least has not altered sufficiently to affect materially the conclusions arrived at. In the case of the States the 1906 death-rate is compared with the insanity figures for 1904. This was unavoidable as no others were obtainable. I do not suppose however that the rates changed so greatly in two years that all value from these comparisons is lost.

In calculating the coefficients of correlation and their probable errors a doubt arose as to whether it was right to give every city the same weight. Should a city with three million inhabitants be given greater weight than one of only 100,000? If it were proved that any factor, such for instance as a meteorological condition, determined the incidence of cancer, it is clear that weighting for size would be wrong. Again if overcrowding was a marked factor it would be unjustifiable to weight for size. To take 100,000 inhabitants as a unit was quite arbitrary; I have therefore regarded each city or State as a unit. This reduces to a minimum the value of the denominator in the probable error, which is therefore of maximum size.

When first approaching this problem I was of opinion that in spite of the trend of modern research it might yet be demonstrated that cancer was of infectious origin, either bacterial or protozoic in nature. Many infectious diseases have a very marked seasonal incidence, and are in certain cases closely correlated with various meteorological conditions. That this is so in respect to enteric fever in the Transvaal I have shown elsewhere, and it is I think reasonable to suppose that if

an organism has an existence apart from its human host it is quite likely that this should be so. Cancer being a disease of fairly long duration, and of uncertain date of onset in relation to the time of death, there was no way of finding out whether the disease was contracted more frequently at one time of the year than another. If therefore meteorological conditions have an influence on causation, it must be sought in place rather than in time. For this purpose I have obtained from the U.S.A. Government publication *Climatology of the United States*, average meteorological conditions for 27 Registration cities with populations of over 100,000 inhabitants.

Corrected Cancer Death-Rate, 27 Cities, 1900—1904.

Sunshine in hours	$r = -\cdot0142 \pm \cdot1323$
Mean Temperature	$r = -\cdot1478 \pm \cdot1294$
Mean Maximum Temperature	$r = -\cdot2236 \pm \cdot1257$
Mean Minimum Temperature	$r = -\cdot0148 \pm \cdot1323$
Rainfall	$r = -\cdot2517 \pm \cdot1239$
Days of Rain	$r = -\cdot1621 \pm \cdot1288$
Absolute Humidity	$r = -\cdot2014 \pm \cdot1269$

Cancer Death-Rate, for the 17 Cities.

Sunshine	$r = \cdot1268 \pm \cdot1659$
Rainfall	$r = \cdot3163 \pm \cdot1518$
Mean Temperature	$r = -\cdot0732 \pm \cdot1685$
Mean Maximum Temperature	$r = -\cdot1509 \pm \cdot1600$
Mean Minimum Temperature	$r = \cdot1254 \pm \cdot1659$
Rel. Humidity 8 a.m. ...	$r = \cdot3101 \pm \cdot1519$
Rel. Humidity 8 p.m. ...	$r = \cdot1603 \pm \cdot1600$
Range of Humidity	$r = \cdot0724 \pm \cdot1685$

It will be observed that out of the seven correlations appearing in the table none of the values are really significant when compared with their probable errors. I had previously calculated the correlations for 17 of the larger cities and values of the same numerical order were obtained, but in many cases the sign was different. They are appended for the purpose of comparison. These values must therefore be considered as zero.

It will be seen from these figures that so far as 27 cities of the U.S.A. are concerned there is no correlation between the average climatic differences obtaining in the city and the prevalence of cancer, as measured by its average death-rate over the five years 1900—1904.

It has frequently been stated that cancer is less prevalent in hot climates, and there are facts to be obtained in support of this general statement. It is not however necessary to assume that this is due directly to climate, and another explanation is, I think, probably the right one. Marked differences of race, temperament, and pressure of life are to be found in tropical and sub-tropical countries, as compared with temperate and cold ones; the exception to this being the case now under discussion, the United States of America.

So far then as these figures go we have no evidence to connect meteorological factors with the variable incidence of cancer. This, although a negative result, is not, I think, without its importance.

Finding that the theory of the infectious origin of this disease gained no support from the values thus discovered, it occurred to me to correlate the cancer rates with those of some other disease. For this purpose I chose diabetes, and for the following reasons. (1) Both diseases have very much the same age distribution. (2) They stand almost alone as being on the increase, while other causes of death show declining rates. (3) The aetiology of both diseases is obscure. (4) Both being diseases of old age, the heredity factor cannot have been increased by the results of modern medical skill, for the prolongation of life that might thus have been procured would have been mainly at a period when the procreative power was passed. In fact the tendency of modern times is rather to postpone marriage to a later period of life, and this might be expected to reduce any heredity factor there may be. (5) If there were a common factor in the causation of the dual increase a correlation between these diseases might be discovered.

There seemed then sufficient justification for undertaking the labour of calculating correction factors for the diabetes rates. As was to be expected both correction factors are similar in value.

Professor Pearson has shown that when correlation tables are formed between rates, in such a way that a common factor occurs in both the variables, a "spurious correlation" is obtained. In this case therefore the usual formula $r = S(xy)/N\sigma_x\sigma_y$ can no longer be employed. On his advice* I have therefore used the following formula when dealing with the correlation as existing between rates:

$$\rho_{xy} = \frac{r_{xy} - r_{zx}r_{zy}}{\sqrt{(1 - r^2_{zx})(1 - r^2_{zy})}}.$$

Here x = the number of deaths from one disease, y = the number from a second, and z = the number of individuals in the district. Throughout this paper where the symbol ρ occurs it refers to values found by this partial correlation formula.

In dealing with diseases like cancer and diabetes, a significant correlation will probably occur when uncorrected deaths are used, due to their similar age incidence. This will be most marked in the value found for the races, and occupation groups, and least so for the cities. This will be seen by comparing the "Coefficient of Variability" for the cancer correction factors for the different groups:

Cities	$V_c = 11{\cdot}46\ ^{\circ}/_{\circ}$.
States	$V_s = 14{\cdot}57\ ^{\circ}/_{\circ}$.
Races	$V_r = 38{\cdot}39\ ^{\circ}/_{\circ}$.
28 Occupation Groups	$V_o = 43{\cdot}86\ ^{\circ}/_{\circ}$.

* [This formula gets rid of the "spurious correlation" due to forming "rates," i.e. the common population totals, but it does not of course get over the high spurious correlation of the so-called "age correction factors." Ed.]

As, therefore, using corrected deaths introduces "spurious correlation" into the value of ρ, and as using uncorrected deaths will introduce a correlation due to the age factor, the values of ρ calculated, both with and without the use of a correction factor, are given*. The symbol ρ' will be used to denote values found from corrected deaths. The necessity for correcting for age is not of such importance when comparing cancer with suicides or insanity.

The partial correlation coefficients found for cancer and diabetes are as follows:

40 Cities, 1900—1904, $\rho = \cdot6896 \pm \cdot0559$, $\rho' = \cdot7325 \pm \cdot0494$.

15 States, 1906, $\rho = \cdot9088 \pm \cdot0303$, $\rho' = \cdot8258 \pm \cdot0554$.

These values were sufficiently striking to lead one to further consider the matter.

From figures obtained from *Vital Statistics* (Vol. I.) U.S.A. Census, 1900, I calculated correction factors for the cancer and diabetes death-rates of the various races residing in the Registration States, and classified according to birth-place of mother. The correlation thus obtained for these two diseases is certainly significant:

$$\rho = \cdot8609 \pm \cdot0552, \quad \rho' = \cdot5442 \pm \cdot1501.$$

That these values are not due to errors of random sampling is shown by their probable errors, for the odds are some millions to one against such an occurrence. Nor does it seem likely, that they are entirely due to "spurious correlation" in the ρ's and to an entirely different source in the case of the ρ's. To what then are these correlations due? It seemed possible that the agreement of rate might be due to certain cities having more efficient registration, although the remarks of the Registrar did not lend much support to this theory. He writes: "The 'registration area'—that is to say, the States having laws, the results of whose operation have been accepted as giving practically complete mortality returns, together with the cities in non-registration States where deaths are satisfactorily registered under local authority—remains substantially the same from 1900 to 1905. The geographic distribution is shown in the accompanying map" (*Mortality Statistics* 1905, *U.S.A.*: see our Fig. 3).

I have already referred to the special returns of deaths made by the Census Department in 1900. The small value of the Coefficient of Variation (2·225 %) in the "percentage error of registration" is I think sufficient evidence that the correlation values are not due to errors in registration. If these high correlations were due to varying efficiency in registration, then other diseases might be expected to show a similar value when correlated with cancer. It will be seen further on that there is no significant correlation between cancer or diabetes

* [I believe I have found a satisfactory method of making *both* age and population corrections free from spurious correlation. This method will shortly be published. ED.]

and diseases of "ill-defined or uncertain cause." Probably one of the most suitable diseases as a control would be nephritis, were it not that its age frequency is so similar to cancer, thus the age factor will give rise to errors to an even greater extent than occurs in the case of diabetes.

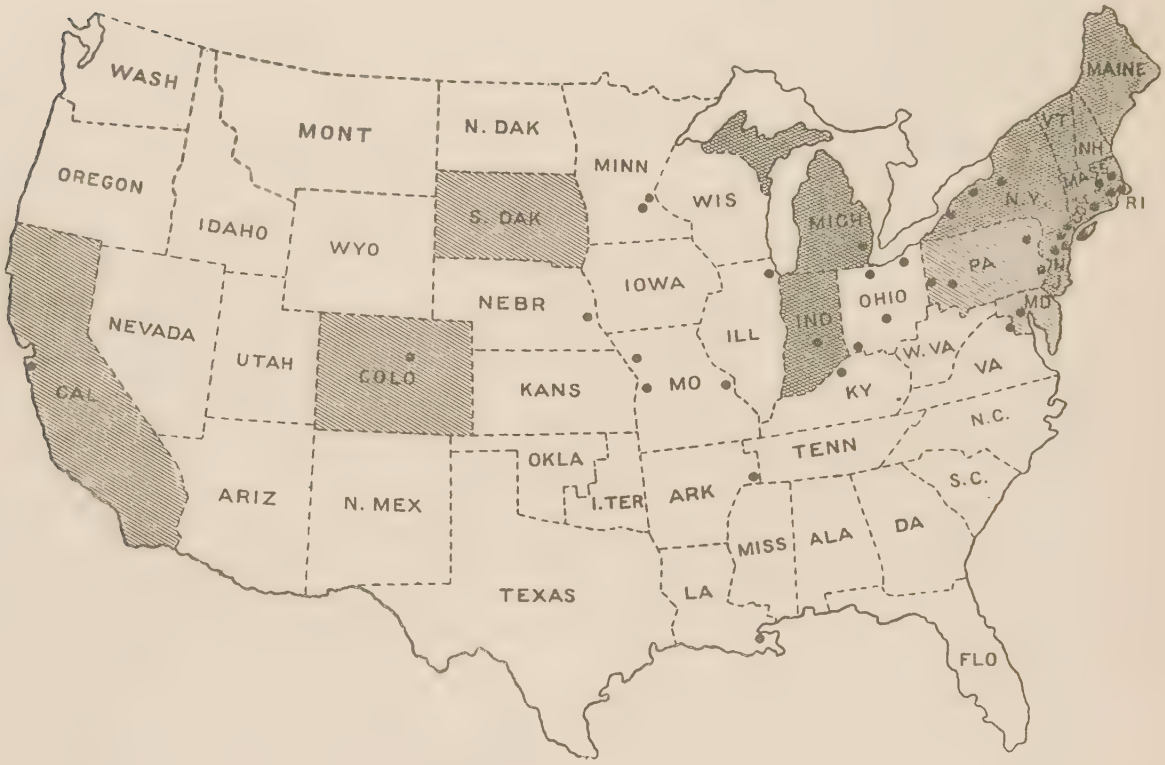


FIG. 3.

The values of ρ and ρ' are given for this disease as they may serve as a useful contrast to the other correlation values. In calculating the value of ρ' the cancer correction factor was used for the nephritis deaths as the age frequencies of the two diseases are not very dissimilar. Cancer and Nephritis, 40 Cities:

$$\rho = \cdot 3792 \pm \cdot 0913, \quad \rho' = \cdot 1571 \pm \cdot 1040.$$

These values are of a quite different order from those found for cancer and diabetes.

Unfortunately no figures were obtainable to allow of a correction being made for those deaths which occurred in hospital and did not belong to the district. Although this correction is most desirable, yet it is not likely to very greatly affect the correlation values for cancer and diabetes, as the latter disease is not one that bulks very largely in hospital deaths. It is not a disease very prevalent among the class of patient that frequent hospital, and from its nature would not be admitted unless combined with some acute affection. This of course will occur in a certain number of cases but to a small extent as compared with cancer. This objection will not apply to the returns from the States, and all the cities here dealt with, except a few eastern ones, will draw from fairly large rural areas.

Before coming to the conclusion that the correlation found between cancer and diabetes is of real significance, one must as far as possible exclude all sources of error that might lead to false deductions. The question of the inaccuracy in the records from incomplete registration has already been discussed, but there is another form of inaccuracy that must be considered, viz. that arising from want of care and knowledge on the part of the certifying medical practitioner. In *Mortality Statistics U.S.A.* 1907, the following remark occurs: "In Table IV. may be found two columns headed 'ill-defined causes' and 'cause unknown' which are of no special significance in themselves, but which are nevertheless important, because they show the margin of understatement that must be considered in examining the rates for more definitely stated causes of death." It seemed therefore desirable to correlate cancer and diabetes death-rates with a five-yearly average rate of these groups, for the 40 cities occurring in the other tables. The values found are as follows:

Cancer and diseases of ill-defined and unknown cause, $\rho = \cdot 1020 \pm \cdot 1055$.

Diabetes and diseases of ill-defined and unknown cause, $\rho = \cdot 0773 \pm \cdot 1060$.

It does not seem likely therefore that any appreciable correlation will have arisen from this cause.

It has been suggested to me that in towns well equipped with competent medical men these diseases would figure less in the death returns, because by means of operative treatment in the case of cancer, and dietetic treatment in the case of diabetes, death might eventually be due to some other cause; whereas where medical skill was inferior, or the population not sufficiently educated to take suitable precautions in regard to diet, or submit to operation, a larger number of cases of both diseases would appear in the records. In the first place I do not suppose that the average medical skill would vary very greatly in cities of the size dealt with in this paper. It is no doubt true that in both diseases, life may be prolonged by skilful treatment but a cure is not effected, except in very rare instances. When death occurs from an intercurrent disease the older complaint is included on the death certificate. At least this should be done and it is less likely to be omitted where the medical skill is of a high order. In classifying causes of

death it is the rule to select the diseases ordinarily known as constitutional in preference to those known as local diseases. This will be made clear by an extract from the New Tables issued by the L.G.B. and the Schedules of causes of death: "Thus Cancer should be selected in preference to Pneumonia, and Diabetes in preference to Heart-disease." Further the groups already dealt with, viz. "Ill-defined and unknown causes" are, perhaps, the best statistical guide to the efficiency of the local medical profession.

Professor Pearson has kindly drawn my attention to another possible source of error. If it were shown that both diseases were more likely to attack persons in the same social class, or engaged in the same occupations, an artificial correlation might then be set up, owing to the inhabitants of the different cities being engaged in occupations of a diverse nature. It will be seen later that cancer and diabetes are probably negatively correlated in respect to the occupational groups. There is I think no doubt that diabetes is a disease especially prone to attack the wealthy and sedentary classes, but in the U.S.A. this does not seem to be true of cancer. This question is of importance because Mr Heron has by a study of the London statistics arrived, by indirect methods, at an opposite conclusion. From the U.S.A. Census Reports it was possible to obtain the numbers of men employed in various occupations, together with their age distribution, and the cancer and diabetes death-rates. These are given first in eight inclusive groups and then in sub-groups. For the present purpose correction factors for the death-rates were calculated for the eight large groups and for 28 sub-groups as shown in the table. The various occupations were then arranged in order according to their social status, but as it was unlikely that two people would place them in exactly the same order I asked a friend to rearrange them for me and the results of both orders are given. The correlation was found by the method of ranks as given by Pearson*, $r = 2 \sin \frac{\pi}{6} \rho_{12}$, where $\rho_{12} = 1 - 6S(\nu_1 - \nu_2)^2/N(N^2-1)$. Correlating high death-rates with superior social occupations the values are as follows:

Eight Inclusive Groups.

Cancer and Occupations $r = -\cdot5176 \pm \cdot1746$.
Diabetes and Occupations $r = \cdot9374 \pm \cdot0289$.

28 *Sub-groups.*

	1st grouping	2nd grouping
Cancer and Occupations	$r = -\cdot4574 \pm \cdot1007$,	$r = -\cdot3680 \pm \cdot1102$.
Diabetes and Occupations	$r = \cdot6716 \pm \cdot0700$,	$r = \cdot6748 \pm \cdot0694$.

To check the results thus found four "four-fold tables" were constructed, the division of the death-rates being taken at their mean, and the occupations being divided at Nos. 9 and 17 of the order as shown in the table.

* "On Further Methods of Measuring Correlation," *Drapers' Research Memoirs*, Dulau and Co.

There seems to be no doubt that in America there is a negative correlation between cancer and better social class, as measured by occupation, while there is a high positive correlation between diabetes and better social conditions. Thus

Cancer and Occupation.

Death-rate.

Social Status	H.	L.	
High	2	7	9
Low	11	8	19
	13	15	28

$r = -\cdot 56 \pm \cdot 09$,

Social Status	H.	L.	
High	5	12	17
Low	8	3	11
	13	15	28

$r = -\cdot 63 \pm \cdot 08$.

Diabetes and Occupation.

Death-rate.

Social Status	H.	L.	
High	9	0	9
Low	3	16	19
	12	16	28

$r = 1 \cdot 0$,

Social Status	H.	L.	
High	11	6	17
Low	1	10	11
	12	16	28

$r = \cdot 82 \pm \cdot 04$.

there does not seem any reason for supposing that an artificial correlation will have arisen between cancer and diabetes death-rates owing to prevalence of varying occupations in the different cities.

If the increased rates observed in the cases of cancer and diabetes were due to a common cause then it is probable that their rates of growth will be found to be fairly highly correlated. Not being able to obtain rates for a sufficient period of time, from the United States reports, the rates given in the Registrar's Report for England and Wales, in five-yearly groups, for the 35 years 1871 to 1905 were used, and here $\rho = \cdot 8060 \pm \cdot 0893$. This high correlation shows I think a strong probability that there is a common factor influencing the increase of both diseases. That this value is explicable on the assumption that the increased rates are merely apparent and due to more careful diagnosis is in view of the facts already mentioned almost inconceivable. It is equally clear that the increase is not due to alteration in the age constitution of the population. For example had the same cancer death-rate been operating in 1851—60 as was so acting from 1891—00 the death-rate would have been 743 per million, and in 1871—80, 749, while in 1891—00 it was 758. Thus in 50 years the increased ratio of old people would have led to a gain in rate of only 15 deaths per million of the population, whereas the recorded increase in the last 35 years 1871—05 is 534 per million. It has been previously pointed out, that no great increase in our knowledge of either disease has recently been made, that would aid the general practitioner in more accurate diagnosis in the final stages of the disease.

If then we have succeeded in excluding artificial sources of correlation, it remains to find, if possible, the factor that is producing the increasing rates in these two diseases, and giving rise to their correlation in the various cities and districts.

Only one cause, it seems to me, will fit the facts as we know them, viz.: the pressure of modern civilisation and the strain of modern competition, or some

factor closely associated with these. Radio-activity and various electric phenomena have from time to time been accused of producing cancer. The increased use of high tension currents is an undoubted fact in modern city life, but a glance at the varying rates as applying to different occupations will suffice to show that the explanation does not lie here. Another marked feature of modern life is the increasing use of preserved foods. But again the death-rate as evidenced by the occupation groups seems to negative this as a probable cause. For instance the agricultural rate is higher than that for the military and police.

The increasing pressure of competition with its attendant nervous strain appears to be the factor in the aetiology of both diseases that will account for the phenomena. It has long been recognised that nervous shock and worry may be a determining cause in diabetes.

At once another disease becomes associated with this factor in modern life. Insanity, also on the increase, should show a positive correlation with cancer.

Unfortunately figures of the insane rates for the various cities in the United States are not obtainable, so on the suggestion of Dr Troup I have correlated suicides as being the nearest approach to a measure of the prevalence of insanity. Doubtless suicides form some guide to the stress and strain of life in the various districts, but other factors have probably also an effect in determining the prevalence of suicide. Religious beliefs, and moral training, may have a marked influence. The correlation found for cancer death-rates and suicides for the 40 cities, for the five-year groups, is:

$$\rho = \cdot 3962 \pm \cdot 0899,$$

and the 15 States for the year 1906:

$$\rho = \cdot 4412 \pm \cdot 1402,$$

and for the Races according to birth-place of mother, 1900:

$$\rho = \cdot 6010 \pm \cdot 1362.$$

Although these values are not high they are possibly significant; and in the case of the races of the same order as that found to exist between the insane enumerated in hospital and suicides for the 15 States which is:

$$\rho = \cdot 6062 \pm \cdot 1101.$$

In the case of correlations found for the States and Races, it must be remembered that much greater fluctuations may be expected owing to the values being calculated on the returns for only one year, while in the case of the Cities it was possible to obtain a steadier result by averaging the returns for five years.

From the special report on *The Insane and Feeble Minded in U.S.A.*, published by the Census Office and dealing with the returns of 1904, I was able to obtain figures for the insane as they apply to the various States.

Particular caution is required in dealing with these figures, and a few extracts from the introduction to the report may enable the reader to estimate better the

true value of the results. The report deals only with the insane in institutions. "No institution for the insane, except a very few small private institutions, refused to give information. Therefore so far as all classes of institutions are concerned the present enumeration is practically complete," p. 6. "To offer any estimate of the probable number of the insane that would have been disclosed by an investigation which included those not in institutions in addition to those in hospital on 31/12/03 would be a mere guess. But it is certainly within the truth to say that there were as many on that date as were found outside of hospitals in 1890, some 30,000," p. 8. This number refers to the whole of the United States and not alone to the 15 States dealt with in this paper. "In many cases the high rank given some States, for instance, Massachusetts, New York, Connecticut, New Jersey, California, &c., probably approximate actual conditions," p. 4.

Two correlation tables were prepared so that the matter might be viewed in somewhat different lights, and as much information as possible obtained from the figures. It seemed probable that yearly admission might be an even better guide to the prevalence of insanity than the actual number in hospital on any given day; because where accommodation was insufficient for the pressure occasioned by cases waiting for admission, it would be possible to mitigate this by discharging chronic and convalescent patients somewhat earlier than would be done by hospital authorities where there was more ample accommodation. If this were so then the admissions would be a better guide to the prevalence of the disorder than the actual number in the institutions. The values of the correlations for cancer death-rates with the insane are as follows:

Insane enumerated 31/12/03	$\rho = \cdot 8711 \pm \cdot 0420.$
Insane admissions 1904	$\rho = \cdot 9173 \pm \cdot 0278.$
Diabetes and Insane admissions	$\rho = \cdot 8074 \pm \cdot 0608.$

These values seem to give additional support to the theory of a nervous factor in the aetiology of cancer, and it would be interesting to find an independent measure of the stress of life in the different districts, that could be used as an index to the intensity of the struggle for existence.

In a general way one would expect that in the larger cities the competition would be greater than in the smaller ones, although this would probably not be true of cities with only small differences in population, and other factors besides mere size would have to be taken into consideration. No small cities occur in the tables here used, and the correlations between population and cancer or diabetes death-rates are scarcely significant:

Cancer death-rate and population	$\rho = \cdot 1049 \pm \cdot 1055.$
Diabetes death-rate and population	$\rho = \cdot 3007 \pm \cdot 0970.$

I thought that a rough measure of the social and economic conditions prevailing in a district might be obtained by reducing the number of periodicals

and newspapers printed in the various States to the rate per head of population, and correlating with cancer death-rate; the value thus found is:

$$\rho = \cdot 6182 \pm \cdot 1076.$$

I record these observations for any interest that they may have, but do not think that the deductions should be pushed too far.

Another aspect of the question that requires investigation is the death-rate among the various occupations. No reliable insanity figures can be obtained that correspond to the grouping for occupations as given in *Vital Statistics*, Vol. I., U.S.A. Census, 1900. In fact the relation of insanity to occupation has not been worked out for the United States. As the Editor of the Report above referred to says "On the whole, the question of the relation of insanity to occupation is one that still awaits systematic treatment. The available means of enquiry do not suffice even for a perfect classification of occupations, much less for obtaining data which permit exact comparisons with the total number of the insane drawn from any given occupation with the total number of the same age, &c., engaged in that particular occupation."

Considerable difficulty arises in correlating cancer and diabetes in respect to occupations owing to the large value of the Coefficient of Variability of the correction factor for age distributions, in the various occupations. An inspection of these factors will show that an appreciable positive correlation may be expected from this cause, on the other hand the value of ρ' will contain a spurious correlation of unknown size. Taking into consideration these facts and also that the negative sign of the correlation as found for the eight groups is unchanged even when uncorrected deaths are used, it will probably be safe to conclude that the correlation between cancer and diabetes death-rates with occupation groups is negative.

Occupations. Cancer and Diabetes.

8 Inclusive Groups	$\rho = -\cdot 0645 \pm \cdot 2375$,	$\rho' = -\cdot 6719 \pm \cdot 1308$.
28 Sub-groups	$\rho = \cdot 2866 \pm \cdot 1170$,	$\rho' = -\cdot 4832 \pm \cdot 0977$.

At first sight this may seem unexpected, but I do not think it is really surprising. The rates of both diseases vary considerably in the different occupations. It must be remembered that certain occupations consist of highly selected individuals, and thus the unstable in certain respects may have been weeded out. Whereas a similar condition imposed on a city or district may lead to an increase in both rates, conditions of work &c. may determine which form the manifestation of instability shall take. For instance occupations associated with a sedentary life probably predispose to diabetes.

The use of an analogy will better explain my meaning. The death-rates from Typhoid Fever and Bullets during time of war would be much increased, and both in relation to time and place would show positive correlation with war. But if

these rates were to be compared in relation to deaths in occupations—soldiers at the front with men in garrison—no positive correlation would be observed. Thus under the heading of occupations we may be measuring rather the determining causes of the type of breakdown, than the fundamental cause of the disease.

I may perhaps be allowed briefly to refer to the theory which has been adopted as a working hypothesis in the present investigation. As far as this investigation is concerned there does not seem to be any evidence as to the infectious nature of cancer. Nor has any evidence been adduced that would lead to this belief with the possible exception of cancer-houses, while on the other hand, all the improvement brought about by public health reform, in the way of reducing over-crowding, abolition of back-to-back houses, &c. has not been followed by any reduction of the disease, on the contrary an actual increase has been taking place. That nervous centres do exercise an extraordinary influence over tissue cells is a matter of common knowledge and that some nervous disturbance may affect the cell metabolism in diabetes, and the cell growth in cancer does not seem to me impossible, even if a novel view. This suggestion does not necessarily exclude the presence of an organism as the determining cause, and several analogies will occur to the reader. For instance in a disease like scurvy, which is almost certainly of infectious origin, the organism does not usually obtain a footing until the soil has been prepared by errors of diet.

It must be understood that the strain occasioned by modern competition is only regarded as an exciting factor, in so far as it leads to a breakdown of nervous origin. This breakdown of course may occur without any undue strain, but will in the aggregate do so more frequently when this factor is present. The heredity factor, i.e. the constitutional weakness, is of course of primary importance.

Problems connected with the occurrence of cancer in animals are at present difficult to deal with statistically. That malignant tumours occur in vertebrates is well known, as the following extract from the Third Report of the Cancer Commission shows:—"Cancer is ubiquitous in man and vertebrate animals. It has been shown that cancerous tissue of a species of animal retains the characters of that species, whereas in infective tumours (e.g. tubercle) occurring naturally in separate species, the biological characters of the newly formed tissue are determined by the common infective agent and not by the tissues of the affected animal."

Again, the question of cancer in African Natives requires more study, and the increasing rates for these races as observed in America are of interest. The phlegmatic nature of the Bantu in Africa is well known. The prevalence of Diabetes among Jews is another problem of interest that awaits explanation. This race, however, seems particularly liable to certain nervous complaints, as for instance various forms of insanity, and Amaurotic Family Idiocy.

Since writing the above I have been looking through the small literature of insanity at my disposal and have been struck with the following sentences:—"It

has been said that Diabetes, or Glycosuria at least is frequent among the insane. There is, however, some connection between Diabetes and Insanity, as we shall proceed to point out. Dr Maudsley has recorded his experience that it is not uncommon to find a history of Diabetes in the parents or near ancestors of insane patients; this is also our experience, especially among the affluent classes." G. H. Savage.

"Dr Snow informs me that independently of actual brain-deposits, insanity is not uncommon among cancer patients, and that the incurable wards of the Cancer Hospital are seldom without two or three cases." (*Dict. Med. Psych.*, Hack Tuke.)

Several considerations have led me to offer this paper for publication in its present form. Firstly, I venture to hope that the subject will be considered of sufficient importance to induce other biometrical workers to assist in its elucidation; secondly, the time at my disposal is limited while much remains to be done; and thirdly, much of the work is impossible to a resident living away from the big libraries, as many books of reference are unobtainable. Even such districts as have been dealt with could be better handled by workers conversant with the districts and their local conditions. Lastly, the publication of a preliminary paper may bring helpful criticism and so enable future work to be more profitably planned.

It would much enhance the value of the Death Reports, if the Registrars could see their way to add special tables of the causes of death, in cases where the diagnosis has been confirmed by post-mortem examination. A list of associated factors found in each case might be added, much on the lines adopted by the London Asylums Board. Thus there would in time grow up a body of statistical material of the utmost value for determining the true age distributions of the various diseases, and for obtaining many other important pathological facts.

I would take this opportunity of thanking Professor Pearson for much kind advice, and also Dr J. M'D. Troup and Mr G. Clark for their assistance during the compilation of this paper.

Conclusions.

1. That recorded differences in cancer and diabetes death-rates, as applying to different Districts and Cities of the U.S.A., as well as the increased rates observed in recent years, do indicate real differences in the prevalence of the disease.

2. The correlations found to exist between cancer, diabetes, and insanity are not fortuitous and due merely to errors of observation or record.

3. The statistics dealt with in this paper do not give any support to the suggestion that cancer is of infectious origin.

4. That whatever theory as to the causation of cancer is adopted, some explanation of the remarkable correlations between cancer, diabetes and insanity is required.

5. The explanation suggested to account for the facts as disclosed by statistical analysis is, that conditions of modern life, acting on physiologically unsound material, may account for the correlations existing between these three diseases, as well as for their increasing rates.

6. If this suggestion be correct, then the study and teaching of Eugenics has an additional purpose. Although no hope for the sick can be derived from this source, yet the future of the race will not be so hopeless, when children are taught the eugenic law, that marriage with the physiologically unfit is as anti-social, as marriage with their social inferiors is at present held to be.

TABLE I.

Cancer Death-Rates.

Age	Male			Female	
	Observations		Calculated Areas	Observations	Calculated Areas
	Record	Altered		Record	
2·5	12	2	1	11	0
7·5	6	4	2	7	0
12·5	6	6	6	7	4
17·5	10	10	12	14	22
22·5	32	32	26	33	69
27·5	48	48	54	116	178
32·5	78	78	110	313	371
37·5	196	196	210	709	680
42·5	374	374	380	1200	1127
47·5	672	672	656	1909	1705
52·5	1120	1120	1068	2483	2418
57·5	1642	1642	1640	3229	3225
62·5	2278	2278	2338	3781	4330
67·5	3100	3100	3064	4549	4854
72·5	3772	3772	3728	5043	5472
77·5	4050	4050	4020	7137	5777
82·5	3730	3730	3796	5654	5621
87·5	2920	2920	3014	4000	4916
92·5	3920	2000	1886	3927	3563
97·5	1600	800	810	1818	1607
102·5	0	200	194	0	0
107·5	0	0	10	0	0
Totals	29566	27034	27025	45940	45940

Male Curve $y = 4050\left(1+\frac{x}{52{\cdot}3713}\right)^{45{\cdot}0100}\left(1-\frac{x}{7{\cdot}0852}\right)^{6{\cdot}1704}$.

Female Curve $y = 5818\left(1+\frac{x}{15{\cdot}2550}\right)^{4{\cdot}0559}\left(1-\frac{x}{4{\cdot}7721}\right)^{1{\cdot}4564}$.

TABLE II.

Diabetes Death-Rates.

Constants of Curves.

	Total Data	Suggested Old Age Mortality
Mean	66·9786	70·7810
Mode	86·0008 (Type J)	75·5300
S. D.	3·2984	2·2603
$\sqrt{\beta_1}$	1·3989	0·7403
β_2	5·1345	3·6254

Age	Observations	Mid-Ordinates	
		Old Age Component	Total Curve
2·5	13	0	8
7·5	19	0	19
12·5	36	0	29
17·5	38	1	38
22·5	36	2	45
27·5	51	5	51
32·5	57	11	57
37·5	65	24	67
42·5	102	49	86
47·5	117	93	122
52·5	204	163	182
57·5	238	269	277
62·5	443	403	403
67·5	579	553	553
72·5	627	662	662
77·5	685	676	676
82·5	585	528	528
87·5	201	252	252
92·5	39	29	29
97·5	3	3	3

Total Curve $y = 2280\left(1+\frac{x}{30{\cdot}9147}\right)^{6{\cdot}1835}\left(1-\frac{x}{0{\cdot}1309}\right)^{-0{\cdot}0262}$

Suggested Old Age Component $= 691{\cdot}7\left(1+\frac{x}{34{\cdot}1585}\right)^{26{\cdot}5162}\left(1-\frac{x}{4{\cdot}0781}\right)^{3{\cdot}1657}$

TABLE III.

England and Wales. 1891—1900.

Age	Cancer Death-Rate (per Million)		Diabetes Death-Rate (per Million)	
	Male and Female	Male	Male and Female	Male
0	30	33	4	5
5	16 } 18	18 } 24	12	12
15	37	41	33	37
25	139	99	49	57
35	646 } 360	384 } 223	71	87 } 70
45	1833	1300	136	161
55	3662 } 2567	3160 } 2032	300 } 201	347 } 234
65	5604	5325	460	559
75	6145 } 5766	5824 } 5461	380 } 438	474 } 536
Total Rate	757	600	75	86

TABLE III *bis.*

Death-Rate (per Million).

England and Wales.

Year	Cancer	Diabetes
1871—75	446	36
1876—80	494	40
1881—85	548	51
1886—90	632	62
1891—95	711	69
1896—00	800	81
1901—05	864	89

TABLE IV.

Meteorological Conditions. Average of about 30 years.

City	Hours Sunshine	Mean Temperature	Mean Maximum Temperature	Mean Minimum Temperature	Rainfall in Inches	Days of Rain ·01 and over	Absolute Humidity in grs.
Baltimore ...	219	55	63	47	43	131	3·71
Boston ...	208	49	57	41	44	130	3·15
Buffalo ...	191	47	54	40	37	170	3·03
Chicago ...	218	48	55	41	33	126	3·17
Cincinnati ...	216	55	64	47	38	134	3·46
Cleveland ...	173	49	57	42	36	159	3·21
Columbus ...	206	52	61	43	37	144	3·37
Denver ...	251	50	63	37	14	80	2·03
Detroit ...	202	48	56	41	32	138	3·11
Indianapolis ...	207	55	61	44	42	136	3·38
Kansas City ...	212	54	63	45	36	104	3·63
Louisville ...	217	57	66	48	44	128	3·67
Memphis ...	227	62	70	53	51	115	6·15
Milwaukee ...	155	45	53	38	31	129	3·06
New Haven ...	231	50	58	41	47	134	3·30
New Orleans ...	198	69	76	62	58	128	5·81
New York ...	209	52	59	45	45	132	3·48
Omaha ...	228	50	60	41	31	100	3·17
Philadelphia ...	215	54	62	46	41	130	3·60
Pittsburg ...	179	53	62	44	37	154	3·46
Rochester ...	185	47	55	39	34	178	3·05
St Louis ...	227	56	64	48	37	115	3·78
St Paul ...	206	45	53	35	29	116	2·68
San Francisco ...	239	56	62	50	23	69	3·82
Toledo ...	201	50	58	42	31	135	3·20
Washington ...	212	55	64	46	43	126	3·78
Atlantic City ...	229	52	58	46	42	125	3·96
Mean ...	209·67	52·41	60·52	44·15	37·63	128·37	3·53
σ ...	20·34	5·09	5·11	5·32	8·56	22·87	·80

TABLE V.

Cancer.

City	Correction Factor	Crude Death-Rate 1900—04	Corrected Death-Rate 1900—04	Population per 1000's 1903
Allegheny ...	1·2349	435	537	138
Baltimore ...	1·0543	752	793	531
Boston ...	1·0456	896	937	581
Buffalo ...	1·1940	687	820	367
Chicago ...	1·3224	635	840	1873
Cincinnati ...	1·0384	781	811	332
Cleveland ...	1·2073	582	703	414
Columbus ...	1·0783	651	702	135
Denver ...	1·0876	655	712	147
Detroit ...	1·1383	709	807	310
Fall River ...	1·2389	608	753	105
Indianapolis ...	1·0313	578	596	200
Jersey City ...	1·2151	516	627	222
Kansas City ...	1·1791	553	652	173
Louisville ...	1·0908	594	648	216
Memphis ...	1·3327	373	497	114
Milwaukee ...	1·2289	678	833	303
Minneapolis ...	1·2289	587	721	238
New Haven ...	·9882	783	774	115
New Orleans ...	1·1035	736	812	301
Brooklyn ...	1·1557	627	725	1280
Manhattan ...	1·2701	743	944	2001
Queens ...	1·1921	474	565	180
Richmond ...	·9947	821	816	71
Newark ...	1·1215	686	769	267
Omaha ...	1·2701	519	659	113
Paterson ...	1·1401	631	719	109
Philadelphia ...	1·0341	699	723	1368
Pittsburg ...	1·3211	519	686	345
Providence ...	·9793	850	832	189
Rochester ...	1·0027	806	808	174
St Joseph ...	1·4585	306	446	110
St Louis ...	1·1610	622	722	612
St Paul ...	1·3051	555	724	183
San Francisco ...	·9546	1240	1184	356
Scranton ...	1·3351	420	561	110
Syracuse ...	·9668	760	735	114
Toledo ...	1·1383	574	653	146
Washington ...	·9439	745	703	293
Worcester ...	1·0385	728	756	124
Mean ...	—	—	735·05	374·00
σ ...	—	—	128·01	449·24

TABLE VI.

City	Diabetes Correction Factor	Diabetes		Suicides 1900—04	Nephritis 1900—04
		Crude Death-Rate 1900—04	Corrected Death-Rate 1900—04		
Allegheny ...	1·1381	52	59	145	574
Baltimore ...	1·0173	99	101	113	1446
Boston ...	1·0013	151	151	136	858
Buffalo ...	1·1194	146	163	111	1012
Chicago ...	1·1773	85	100	228	1145
Cincinnati ...	1·0081	90	91	217	1256
Cleveland ...	1·1261	59	66	158	840
Columbus ...	1·0260	83	85	139	702
Denver ...	1·0331	121	125	239	1098
Detroit ...	1·0807	113	122	112	751
Fall River ...	1·1450	114	131	61	1059
Indianapolis ...	·9907	96	95	110	668
Jersey City ...	1·1346	83	94	174	1384
Kansas City ...	1·0791	65	70	161	1080
Louisville ...	1·0388	75	78	127	1043
Memphis ...	1·1867	46	55	79	1370
Milwaukee ...	1·1556	94	109	215	623
Minneapolis ...	1·1322	79	89	135	721
New Haven ...	·9817	151	148	158	966
New Orleans ...	1·0699	61	65	137	2065
Brooklyn ...	1·0885	113	128	19	1740
Manhattan ...	1·1504	152	175	225	2218
Queens ...	1·1261	94	106	216	1993
Richmond ...	·9855	144	142	152	1663
Newark ...	1·0623	112	119	238	1533
Omaha ...	1·1468	64	73	142	602
Paterson ...	1·0791	93	100	90	1005
Philadelphia ...	1·0000	98	98	135	1492
Pittsburg ...	1·1811	47	55	155	767
Providence ...	·9628	162	156	110	1324
Rochester ...	·9817	135	132	137	885
St Joseph ...	1·2542	46	58	105	486
St Louis ...	1·0885	68	74	258	1355
St Paul ...	1·1811	74	87	125	724
San Francisco ...	·9317	176	164	550	1070
Scranton ...	1·2136	93	113	76	972
Syracuse ...	·9566	116	111	105	890
Toledo ...	1·0823	106	115	139	645
Washington ...	·9282	107	99	149	1103
Worcester ...	1·0067	131	132	101	713
Mean ...	—	—	107·02	154·44	1117·15
σ ...	—	—	32·61	80·82	436·58

TABLE VII.

States Registration.

	Cancer			Diabetes			Suicides
	Correction Factor	Death-Rate, 1906		Correction Factor	Death-Rate, 1906		
		Crude	Corrected		Crude	Corrected	
Maine ...	·7081	862	610	·7622	168	128	104
New Hampshire ...	·7135	892	636	·7653	169	129	86
Vermont ...	·7049	853	601	·7622	183	139	140
Massachusetts ...	·9087	903	820	·9135	161	147	96
Rhode Island ...	·9415	783	737	·9387	163	153	153
New York ...	·9510	762	725	·9422	160	151	144
Connecticut ...	·8731	806	703	·8897	188	167	138
New Jersey ...	1·0079	661	666	·9894	128	127	154
Michigan ...	·9334	676	631	·9434	137	129	98
Indiana ...	·9780	537	525	·9740	110	107	116
Maryland ...	1·0384	601	624	1·0176	96	98	89
Pennsylvania ...	1·0485	607	636	1·0218	108	110	113
South Dakota ...	1·2308	354	436	1·1645	88	102	69
Colorado ...	1·1828	513	607	1·1029	84	93	190
California ...	·8310	920	764	·8532	161	137	289
Mean ...	—	—	648·07	—	—	127·80	131·93
σ ...	—	—	91·94	—	—	21·30	52·26

TABLE VIII.

States Registration.

	Insane (per million)		Newspapers to Persons
	Enumerated 31/12/03	Admitted 1904	
Maine ...	125	47	·920
New Hampshire ...	117	83	·522
Vermont ...	255	77	·561
Massachusetts ...	289	133	2·270
Rhode Island ...	235	92	·500
New York ...	339	86	4·510
Connecticut ...	293	89	·505
New Jersey ...	238	65	1·612
Michigan ...	216	58	·988
Indiana ...	166	53	·841
Maryland ...	202	66	·573
Pennsylvania ...	173	50	1·837
South Dakota ...	141	32	·586
Colorado ...	129	86	·984
California ...	361	98	1·025
Mean ...	218·60	74·13	1·22
σ ...	75·50	24·17	1·02

TABLE IX.

Registration Districts. Death-Rate according to Birthplace of Mother in United States of America.

	Cancer			Diabetes			Suicides.
	Correction Factor	Crude Death-Rate	Corrected Death-Rate	Correction Factor	Crude Death-Rate	Corrected Death-Rate	
United States of America ...	·9276	483	448	·8380	96	80	68
Ireland	·8149	764	623	·7239	104	75	61
Germany	·8592	782	672	·7653	122	93	193
England and Wales ...	·7400	720	533	·6818	121	82	104
Canada	1·3591	403	548	1·0653	63	67	65
Scandinavia	1·4474	311	450	1·1161	33	37	107
Scotland	·6901	818	565	·6649	123	82	116
Italy	1·6935	228	386	1·2438	26	32	51
Russia and Poland	2·0626	257	530	1·4296	33	47	58
France	·6421	928	596	·6000	129	77	220
Mean	—	—	535·10	—	—	67·20	—
σ	—	—	82·67	—	—	19·96	—

TABLE X.

Occupations. United States of America, 1900. *Registered States.*

	Cancer			Diabetes		
	Correction Factor	Crude Death-Rate	Corrected Death-Rate	Correction Factor	Crude Death-Rate	Corrected Death-Rate
Professional	·6764	517	350	·6771	222	150
Officials, Clerks, etc. ...	·8167	377	308	·7762	181	140
Mercantile Trading, etc. ...	·7472	520	388	·7257	194	139
Public Entertainers ...	·7642	353	270	·7251	159	115
Service, Military, Police ...	·7075	429	303	·6986	87	61
Manufacturing	·7653	533	408	·7401	112	83
Agriculture	·5484	692	379	·5960	133	79
Labourers not Agricultural ...	·8054	665	536	·7678	79	61
Mean	—	—	350·22	—	—	104·44
σ	—	—	99·54	—	—	36·19

TABLE XI.

	Cancer			Diabetes		
	Correction Factor	Death-Rate	Corrected Death-Rate	Correction Factor	Death-Rate	Corrected Death-Rate
Bankers ...	·3617	414	150	·4350	207	90
Law ...	·5234	525	275	·5669	315	179
Physicians ...	·5355	675	361	·5741	169	97
Clergy ...	·4282	1022	438	·4903	341	167
Officials ...	·8168	377	308	·7762	181	140
Hotel Keepers ...	·4483	601	269	·5059	250	126
Book Keepers ...	1·3172	280	369	1·0238	158	162
Collectors ...	·5344	581	310	·5780	216	125
Merchants ...	·5539	813	450	·5898	310	183
Butchers ...	·9094	445	405	·8159	78	64
Bakers ...	·9422	510	481	·8350	128	107
Police ...	·4814	603	290	·5328	93	50
Printers ...	1·2668	221	280	·9965	37	37
Tailors ...	·8402	584	491	·7804	143	112
Barbers ...	1·2119	275	333	·9556	50	48
Plumbers ...	1·4966	82	123	1·0777	62	67
Bootmakers ...	·6656	341	227	·6788	62	42
Carpenters ...	·4841	777	376	·5375	83	45
Farmers ...	·4525	877	397	·5206	165	86
Servants ...	1·2513	443	554	·9817	74	73
Sailors ...	·5942	1131	672	·6255	168	105
Blacksmiths ...	·5828	1073	625	·6143	123	76
Masons ...	·5210	980	511	·5665	181	103
Iron-workers ...	1·0422	315	328	·8848	29	26
Draymen ...	1·0106	226	228	·8687	59	51
Labourers ...	·7792	691	538	·7511	79	59
Factory Operators ...	1·3636	345	470	1·0565	40	42
Pedlars ...	·7416	627	465	·7257	60	44

TABLE XII.

Correlation Values.

Corrected Cancer Death-Rate and Meteorological Factor.
In 27 Cities, 1906.

Sunshine	$r = -·0142 \pm ·1323$
Mean Temperature	$r = -·1478 \pm ·1294$
Mean Maximum Temperature	$r = -·2236 \pm ·1257$
Mean Minimum Temperature	$r = -·0146 \pm ·1323$
Rainfall	$r = -·2517 \pm ·1239$
Days of Rain	$r = -·1621 \pm ·1288$
Absolute Humidity	$r = -·2014 \pm ·1269$

TABLE XII.—(*continued*).

In 17 Cities, 1906.

Sunshine	$r = \cdot1268 \pm \cdot1659$
Rainfall	$r = \cdot3163 \pm \cdot1518$
Mean Temperature	$r = -\cdot0032 \pm \cdot1685$
Mean Minimum Temperature	$r = \cdot1254 \pm \cdot1659$
Mean Maximum Temperature	$r = -\cdot1509 \pm \cdot1600$
Relative Humidity 8 a.m. ...	$r = \cdot3101 \pm \cdot1519$
Relative Humidity 8 p.m. ...	$r = \cdot1603 \pm \cdot1600$
Range of Humidity	$r = \cdot0724 \pm \cdot1685$

Correlations, 15 *States,* 1906.

Cancer and Diabetes, 1906	$\rho = \cdot9088 \pm \cdot0303$,	$\rho' = \cdot8258 \pm \cdot0554$
Cancer and Suicides	$\rho = \cdot4412 \pm \cdot1402$,	$\rho' = \cdot3801 \pm \cdot1490$
Cancer and Insane Enumerated, 31/12/03	$\rho = \cdot8711 \pm \cdot0420$,	$\rho' = \cdot8222 \pm \cdot0564$
Cancer and Insane Admissions, 1904 ...	$\rho = \cdot9173 \pm \cdot0278$,	$\rho' = \cdot9179 \pm \cdot0274$
Cancer and Newspapers to persons, 1900	$\rho = \cdot6182 \pm \cdot1076$,	$\rho' = \cdot5686 \pm \cdot1178$
Diabetes and Insane Admissions ...	$\rho = \cdot8074 \pm \cdot0608$,	$\rho' = \cdot7906 \pm \cdot0653$
Suicides and Insane Enumerated ...	$\rho = \cdot6062 \pm \cdot1101$	

Correlations, Cities, 1900—1904.

Cancer with Diabetes, 1900—04	$\rho = \cdot6896 \pm \cdot0559$,	$\rho' = \cdot7325 \pm \cdot0494$
Cancer with Nephritis, 1900—04	$\rho = \cdot3792 \pm \cdot0913$,	$\rho' = \cdot1571 \pm \cdot1040$
Cancer with Suicides, 1900—04	$\rho = \cdot3962 \pm \cdot0899$,	$\rho' = \cdot6895 \pm \cdot0559$
Cancer with Population, 1900	$\rho = \cdot1049 \pm \cdot1055$,	$\rho' = \cdot4792 \pm \cdot0822$
Diabetes with Population, 1900	$\rho = \cdot3007 \pm \cdot0970$,	$\rho' = \cdot4084 \pm \cdot0889$
Cancer with Ill-defined Causes	$\rho = \cdot1020 \pm \cdot1055$,	$\rho' = -\cdot3453 \pm \cdot0939$
Diabetes with Ill-defined Causes	$\rho = \cdot0773 \pm \cdot1060$,	$\rho' = -\cdot0193 \pm \cdot1066$
Cancer with Percentage Error of Registration	$\rho = \cdot0526 \pm \cdot1064$	

Cancer Death-Rate according to Birthplace of Mother, United States of America, 1900, *with*

Diabetes	$\rho = \cdot8609 \pm \cdot0552$,	$\rho' = \cdot5442 \pm \cdot1501$
Suicides	$\rho = \cdot6010 \pm \cdot1362$,	$\rho' = \cdot6731 \pm \cdot1166$

Cancer Death-Rate according to Occupation, United States of America, 1900, *with*

Diabetes (8 Large Groups)	$\rho = -\cdot0645 \pm \cdot2357$,	$\rho' = -\cdot6719 \pm \cdot1308$
,, (28 Sub-groups)	$\rho = \cdot2866 \pm \cdot1170$,	$\rho' = -\cdot4832 \pm \cdot0977$

Occupation (Social Rank) with

Cancer (8 groups)	$r = -\cdot5176 \pm \cdot1746$
Diabetes (8 groups)	$r = \cdot9374 \pm \cdot0289$
Cancer (28 groups)	$r = -\cdot4574 \pm \cdot1007$
Diabetes (28 groups)	$r = \cdot6716 \pm \cdot0700$

Death-Rate of Cancer by 5-yearly periods, 1871—75 *to* 1901—05, *England and Wales, with*

Diabetes Death-Rate $\rho = \cdot8060 \pm \cdot0893$

A QUANTITATIVE STUDY OF THE MORPHOLOGY OF THE FRUIT OF THE BLOODROOT, SANGUINARIA CANADENSIS.

By J. ARTHUR HARRIS, Ph.D., Carnegie Institution of Washington.

I. Introduction.

Heretofore investigations in variation and correlation carried out by means of modern quantitative methods have been largely for the purpose of throwing light upon the problems of evolution. But for several years I have been convinced that the methods of higher statistics should find an extensive application in the fields of morphology and physiology.

The present memoir is one of a series in course of preparation in which an attempt has been made to apply the statistical methods to the problems of the morphology and physiology of the inflorescence and fruit. The results set forth will not show their full significance until the constants obtained for other forms are laid beside them for comparison. These data will be ready shortly, but it seems best not to anticipate the results from material not completely worked up.

II. Material and Methods.

(a) *The Morphology of the Fruit of Sanguinaria.*

Sanguinaria, a monotypic genus of the Papaveraceae, widely distributed over the eastern portion of North America, is so familiar to botanists that a detailed discussion of the superficial form of the fruit borne on its slender pedicel is hardly necessary. Gray* has given the essentials when he writes:—

"Ovary oblong, one-celled, with two parietal placentae; style short, columnar; stigma broad, sulcate—two-lobed, the lobes alternate with the placentae. Ovules very numerous, horizontal in several rows on the two placentae, anatropous.

* Gray, A., *Gen. Pl. U. S.* Vol. I. p. 115, 1848.

"Capsule oblong, somewhat compressed, herbaceo-coriaceous, many-seeded, pointed by the short, persistent style, two-valved; the valves separating from the replum formed by the intervalvular filiform placentae."

FIG. 1. Diagram of fruit of *Sanguinaria*, opened in the upper part on one side, showing two placentae with matured seeds and abortive ovules.

I am unaware of any investigation of the embryological development of the ovary of *Sanguinaria*. Until such data are available we shall not be able to say more concerning the morphology of the fruit than Gray did sixty years ago. The essential points to be remembered in the present work are the following. The fruit is borne on a slender pedicel sent up from the root-stalk. It is one-celled with two parietal placentae on which the seeds are arranged in two or three irregular rows. Judging by analogy with other ovaries whose embryology is known, it would seem probable that each placenta is composed of the margins of two distinct carpels.

(*b*) *Collection of Material and Characters considered.*

In the spring of 1906 I collected at Meramec Highlands, near St Louis, Mo., a series of 1000 fruiting stalks of *Sanguinaria*. At the time, the stalks seemed to have their full growth, so far as could be judged by general criteria, but the seeds were not fully mature.

In the spring of 1907 another lot of fruits was secured from the same habitat. Only 400 were gathered, and in this case the peduncle was not taken. This second collection was made considerably later in the season than the first, and the fruits were sensibly larger, as will appear when the means of fruit lengths are considered.

From these collections the following measurements and countings were made:—

1. Length of Peduncle.
2. Length of Fruit.
3. Seeds developing on first placenta.
4. Ovules formed on first placenta.
5. Seeds developing on second placenta.
6. Ovules formed on second placenta.
7. Aborted ovules on first placenta.
8. Aborted ovules on second placenta.
9. Total seeds developing per fruit.
10. Total ovules formed per fruit.
11. Total aborted ovules per fruit.

Some words of explanation concerning these characters are necessary.

1. *Length of Peduncle.* A study of the length of the peduncle was not a primary purpose of this investigation, but it was taken up in a rough and ready manner to gain some general appreciation of the influence which it might have upon the magnitude of the characters of the fruit. It is idle to expect to attain great precision in the investigation of a character of this kind.

Sanguinaria is a perennial plant and we do not know the age of the individual root-stalks which produced these peduncles and fruits. The plants were very probably of different ages; all that we can say with certainty is that they were mature enough to produce fruiting stalks. It is quite possible that the age of the individual has a material influence upon the character of the flowering stalk which it produces.

There is unquestionably some error in the determination of the length of the stalks due to the method of collecting. They were simply carefully pulled from the rhizome. Considering the nature of the material with which we are dealing, I think the determinations of stalk length were quite accurate enough for practical purposes.

2. *Length of Fruit.* In both collections the length of the fruit was taken to the nearest millimetre.

As a character for investigation the length of the fruit presents some serious difficulties. The fruit is not infrequently somewhat curved or otherwise irregular in form, and there is relatively a large possible error in measurement. The fruits collected in 1906 had not quite attained their full size, but I was afraid to leave the collection until later because of the difficulty of counting the seeds which very easily become detached. In 1907 the material had more nearly attained its full development.

To reduce the distributions to somewhat more workable limits the fruits have been combined in groups of 3 mm. range. These have proved very satisfactory working units. Constants calculated from them are essentially the same as those calculated from the ungrouped distributions, as I have assured myself by actually carrying out the arithmetic.

3—11. *Meristic Characters of the Fruit.* The counting of the number of ovules formed the number of seeds developing, and the number of abortive ovules is not at all difficult in this species. The fruits were of slightly different ages in the two collections, but in both cases they were old enough for the determination of the number of seeds developing to be entirely trustworthy.

In recording these characters the numbers on each placenta were noted separately. In the following pages the placentae are referred to as "first" and "second." The two are not differentiated at all, the "first" placenta being merely the first one counted when the fruit was opened. The constants for the distribution of ovules, seeds and aborted ovules should be the same for the two placentae within the limits of the probable error of random sampling. In investigating many relationships the tables may be made symmetrical, that is to say each placenta may be used once as the "first" and once as the "second" number of a pair. The advantage of using these symmetrical tables has been several times pointed out in biometric literature*.

I have found it rather an advantage to work with ovules, seeds and aborted ovules. Naturally, the seeds developing and the aborted ovules per fruit equal the number of ovules formed, and the tables may easily be verified if all three characters be used. Characters 9, 10 and 11 are obtained by adding 3 and 5, 4 and 6, 7 and 8.

(c) *Method of Reduction of Data.*

Sheppard's correction was used throughout in the calculation of the standard deviations.

In calculating the probable errors where more than one organ from each individual is measured, there is always some question as to what value of N shall be used. I have taken N as the actual number of flowering stalks in all cases in which one of the characters involved in the constant was measured only a single time for each inflorescence. For instance in correlating between length of fruit and number of seeds per placenta, or between the fertility characters of the two placentae of the same fruit, N was used as 1000 and 400 in the two

* For instance, by Pearson and his assistants in their memoir on homotyposis in the vegetable kingdom (*Phil. Trans. Roy. Soc. Lond.* A, Vol. CXCVII. pp. 285—379); by Pearl on Paramecium (*Biometrika*, Vol. v. pp. 249—251, 1907); by Pearson and Barrington in their memoir on the inheritance in vision (*Francis Galton Lab. for Nat. Eugenics*, 5, 1909); by Harris (*Biometrika*, Vol. VII. pp. 214—218, 1909).

collections. But when the constants are calculated for the repeated organs it seems logical to calculate the probable errors on the basis of the actual number of such organs, and this was done for the constants calculated for the characters of the individual placentae.

III. PROBLEMS CONSIDERED.

1. *Type and Variability of the Characters Investigated in the Two Collections.*

The fundamental physical constants are given in Tables 1 and 2. It has seemed best to table each collection separately and to put them side by side for comparison in a third table. The distributions upon which these constants are based will be found as the totals of the correlation tables. The differences of the constants to be compared are given with their probable errors in Table 3.

TABLE 1.

Constants for 1906 *Collection of Sanguinaria.*

Character	Average and Probable Error	Standard Deviation and Probable Error	Coefficient of Variation
Length of Stalk in 5 cm. Units*	17·090 ± ·058	2·736 ± ·041	16·01
Length of Fruit in 3 mm. Units†	39·703 ± ·134	6·307 ± ·095	15·88
Ovules on First Placenta	14·980 ± ·097	4·569 ± ·069	30·50
Ovules on Second Placenta	14·924 ± ·099	4·658 ± ·070	31·21
Ovules on First and on Second Placenta	14·952 ± ·070	4·614 ± ·049	30·86
Total Ovules per Fruit	29·904 ± ·193	9·054 ± ·136	30·28
Seeds on First Placenta	11·584 ± ·083	3·872 ± ·058	33·42
Seeds on Second Placenta	11·471 ± ·085	3·977 ± ·060	34·67
Seeds on First and on Second Placenta	11·527 ± ·059	3·925 ± ·042	34·05
Total Seeds per Fruit	23·055 ± ·161	7·531 ± ·114	32·66
Aborted Ovules on First Placenta...	3·396 ± ·060	2·827 ± ·043	83·25
Aborted Ovules on Second Placenta	3·453 ± ·060	2·802 ± ·042	81·14
Aborted Ovules on First and on Second Placenta	3·424 ± ·042	2·812 ± ·030	82·12
Total Aborted Ovules per Fruit	6·849 ± ·110	5·162 ± ·078	75·36

* Constants tabled in units of 1 cm.

† Constants tabled in units of 1 mm.

Another constant which is of much service in studies of fertility is not given in either of these tables. This is the ratio of the total seeds developing to the total ovules formed.

Several years ago Pearson‡ defined fecundity as the ratio of the number of offspring actually produced to those which might have come into existence under the circumstances. His definition was formulated for use in a study of

‡ *Phil. Trans. Roy. Soc. Lond.* A, Vol. cxcii. p. 261, 1899.

inheritance in thoroughbred horses. It seems to me a good one and I shall use it throughout a series of studies on fertility in plants, modifying the terms involved as may be necessary but keeping always to the general idea of the definition. Mayer* used the same ratio in a study of symmetry and fertility in a Medusa. He used the term "coefficient of fertility." Coefficient of fecundity is perhaps a more suitable term, agreeing with Pearson's terminology. The coefficient of fecundity is a very useful constant. Its value lies between 0 and 1, and its form is therefore similar to that of some other statistical constants.

TABLE 2.

Constants for 1907 *Collection of Sanguinaria.*

Character	Average and Probable Error	Standard Deviation and Probable Error	Coefficient of Variation
Length of Fruit in 3 mm. Units†	51·865 ± ·268	7·948 ± ·190	15·32
Ovules on First Placenta	12·812 ± ·137	4·052 ± ·097	31·63
Ovules on Second Placenta	12·792 ± ·136	4·024 ± ·096	31·46
Ovules on First and on Second Placenta ...	12·802 ± ·096	4·038 ± ·068	31·54
Total Ovules per Fruit	25·605 ± ·265	7·850 ± ·187	30·66
Seeds on First Placenta	9·717 ± ·134	3·980 ± ·095	40·96
Seeds on Second Placenta	9·795 ± ·135	3·991 ± ·095	40·74
Seeds on First and on Second Placenta	9·756 ± ·095	3·986 ± ·067	40·85
Total Seeds per Fruit	19·512 ± ·258	7·660 ± ·183	39·26
Aborted Ovules on First Placenta	3·095 ± ·086	2·559 ± ·061	82·68
Aborted Ovules on Second Placenta	2·997 ± ·088	2·610 ± ·062	87·09
Aborted Ovules on First and on Second Placenta	3·046 ± ·062	2·585 ± ·044	84·86
Total Aborted Ovules per Fruit	6·092 ± ·161	4·785 ± ·114	78·55

† Constants tabulated in units of 1 mm.

In the present case the coefficient of fecundity is the ratio of the total seeds developing in the population to the total ovules produced.

For the present series of data we have:—

$$\text{For 1906 series, C.F.} = \frac{23055}{29904} = \cdot7710 \pm \cdot0016‡,$$

$$\text{For 1907 series, C.F.} = \frac{7805}{10242} = \cdot7621 \pm \cdot0042,$$

$$\text{Difference,} \qquad -\cdot0089 \pm \cdot0045.$$

* Mayer, A. G., *Sci. Bull. Brook. Mus.* Vol. I. 1901; see also C. B. Davenport, *Biometrika*, Vol. I. pp. 255, 256, 1902.

‡ It is known that if y_s be the frequency of any class of individuals in a population of m individuals, the probable error of the frequency is given (*Biometrika*, Vol. II. p. 274, 1903) by the formula

$$Ey_s = \cdot67449\sqrt{y_s \times \left(1 - \frac{y_s}{m}\right)}.$$

In the present case y_s is the number of seeds developing in the population m, or total number of ovules. From the absolute probable error of the number of seeds developing calculated from this formula, the probable error of the coefficient of fecundity was obtained by taking the ratio of the absolute probable error to m, the total number of ovules.

In the present paper the study of variation has been a means to an end. The standard deviations have been necessary in the calculation of the coefficients of correlation, and the consideration of the interrelationships between different parts of the organism as indicated by the coefficient of correlation has been the main purpose of the paper.

TABLE 3.

Comparison of Constants for 1906 *and* 1907 *Collections of Sanguinaria.*

Character	Average and Probable Error	Standard Deviation and Probable Error	Coefficient of Variation
Length of Fruit, 1906	39·703 ± ·135	6·307 ± ·095	15·89 ± ·23
„ „ 1907	51·865 ± ·268	7·948 ± ·190	15·33 ± ·37
Difference	+12·162 ± ·300	+1·641 ± ·212	− ·56 ± ·43
Ovules per Placenta, 1906	14·952 ± ·070	4·613 ± ·049	30·86 ± ·51
„ „ 1907	12·803 ± ·096	4·038 ± ·068	31·64 ± ·83
Difference	−2·149 ± ·119	− ·575 ± ·084	+ ·78 ± ·98
Total Ovules per Fruit, 1906	29·904 ± ·193	9·055 ± ·137	30·28 ± ·50
„ „ „ 1907	25·605 ± ·265	7·850 ± ·187	30·66 ± ·80
Difference	−4·299 ± ·328	−1·205 ± ·232	+ ·38 ± ·94
Seeds per Placenta, 1906	11·527 ± ·059	3·925 ± ·042	34·05 ± ·57
„ „ 1907	9·756 ± ·095	3·986 ± ·067	40·85 ± 1·13
Difference	−1·771 ± ·112	+ ·061 ± ·079	+6·80 ± 1·26
Total Seeds per Fruit, 1906	23·055 ± ·161	7·531 ± ·114	32·66 ± ·54
„ „ „ 1907	19·513 ± ·258	7·660 ± ·183	39·26 ± 1·07
Difference	−3·542 ± ·304	+ ·129 ± ·215	+6·60 ± 1·20
Aborted Ovules per Placenta, 1906 ...	3·424 ± ·042	2·812 ± ·030	82·12 ± 2·69
„ „ „ 1907 ...	3·046 ± ·062	2·585 ± ·044	84·87 ± 3·17
Difference	− ·378 ± ·075	− ·227 ± ·053	+2·75 ± 4·15
Total Aborted Ovules per Fruit, 1906 ...	6·849 ± ·110	5·161 ± ·078	75·36 ± 2·43
„ „ „ „ 1907 ...	6·093 ± ·161	4·785 ± ·114	78·54 ± 2·73
Difference	− ·756 ± ·195	− ·376 ± ·141	+3·18 ± 3·65

The constants for the two years considered individually require no separate discussion. The means are not of interest except in comparison with the same constants for other races of *Sanguinaria*. The standard deviations and coefficients of variation are of about the order generally found for plant characters.

The comparison table shows that some of the differences are several times their probable errors, and hence clearly significant. For length of fruit this is quite obviously due to the somewhat greater maturity of the 1907 series. Here I do not care to discuss in detail the interpretation to be assigned to these

differences. I must point out, however, that the fact that our probable errors show that there are significant differences in the samples with which we are dealing does not justify us in concluding that there are differences in the populations which they represent unless we are quite sure that our samples have been drawn in precisely the same way. Rocks, steep banks and gentle slopes, fallen logs and drifts of leaves are possibly all factors which make for heterogeneity in the environment of the individual organism in a plant formation which the ecologist would consider quite homogeneous. In collecting two samples *a* and *b* from such a habitat we might get constants differing sensibly with regard to their probable error merely by failing to have the several subhabitats contribute the same proportional number of individuals in each case.

These points are emphasized because I do not wish to have these results interpreted as indicating that there is a real difference between the whole population of *Sanguinaria* at Meramec Highlands in the two years in question due to some peculiarity in the seasons themselves. There *may* be such a difference, but there is no strong evidence for such a conclusion in our data which were not collected in a way, nor for the purpose, to determine whether there is a real difference between the whole populations in different seasons.

Turning aside from the contrasting of the constants for individual characters and taking a general view of the whole comparison table, I think the impression must be that the agreement between the two collections is very close.

I think we may consider the two series of material upon which we propose to investigate certain morphological relationships as on the whole very similar in nature. Tests for the similarity of series of material used in morphological work have rarely been applied, but it would be desirable if some attention could be given to points like this.

2. *The Correlation between the Length of the Peduncle and the Length of the Fruit.*

It seems important to obtain some idea of the relationship between the degree of development of the fruit-stalk and the size attained by the fruit. The data appear in Table I, 1906, and I have calculated both the coefficient of correlation, r, and the correlation ratio, η.

This latter constant, recently introduced by Pearson*, furnishes an adequate description of the interdependence of two variables when the rate of change of the dependent variable cannot be expressed by the slope of a straight line.

The two constants have the values

$$r = \cdot 335 \pm \cdot 019$$

$$\eta = \cdot 379 \pm \cdot 018\dagger$$

$$\eta - r = \cdot 044$$

* Pearson, K., *Drapers' Co. Research Memoirs*, Biometric Series, No. 2, Dulau & Co., 1905.

† Here E_η was calculated by Pearson's short formula, *loc. cit.* p. 20, 1905.

It appears, therefore, that there is a very substantial interdependence between the length of the peduncle and the length of the fruit. It will be borne in mind, however, that the fruit in this series had not quite reached its maximum size, and that owing to difficulties inherent in the species, constants involving peduncle length and fruit length will always be open to some question.

Blakeman* has shown that, if $\xi = \eta^2 - r^2$, a good test for linearity of regression is given by using the formula

$$\frac{\xi}{E_\xi} = \frac{\sqrt{N}}{0{\cdot}67449} \cdot \frac{1}{2} \sqrt{\xi} \frac{1}{\sqrt{1+(1-\eta^2)^2-(1-r^2)^2}}.$$

How large the value of ξ/E_ξ shall be to be considered significant must be decided by each writer for himself, but when we find $\xi/E_\xi > 2{\cdot}5$ we may begin to be suspicious of the linearity of our regressions. From our data we find

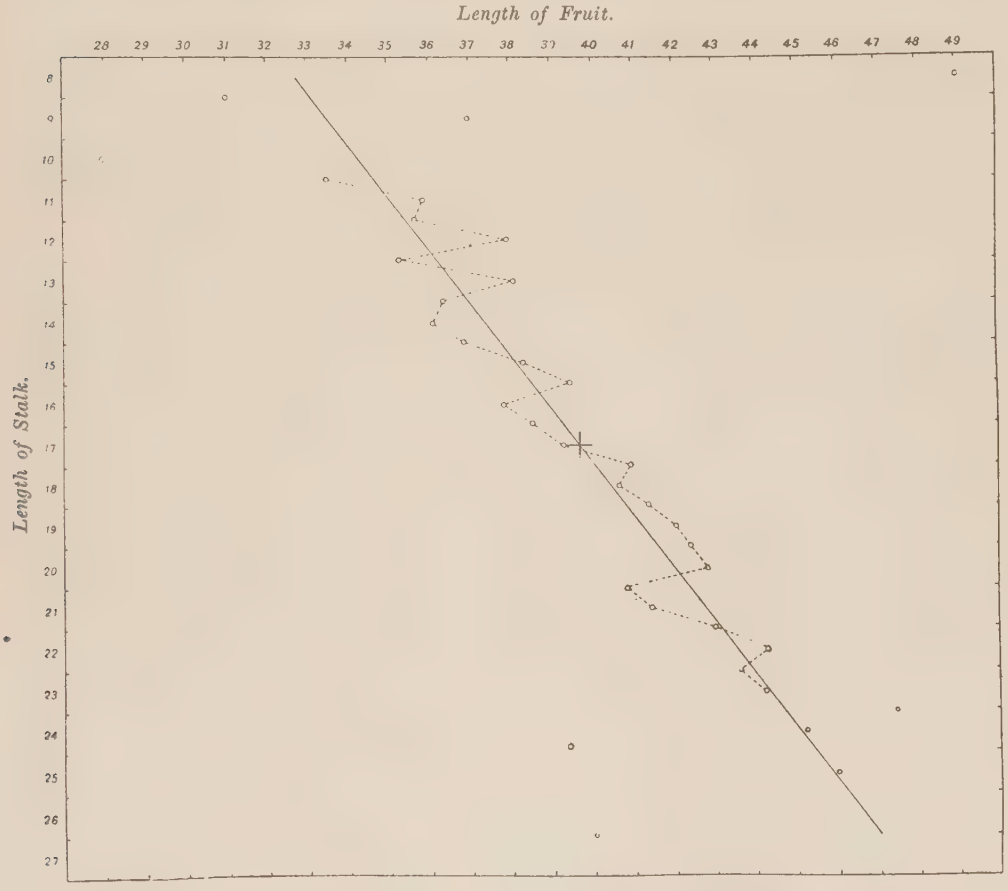


DIAGRAM I. Regression of fruit length on peduncle length. (Means depending on a few isolated observations not joined up.)

* Blakeman, J., *Biometrika*, Vol. IV. pp. 335—350, 1905.

$\xi = \cdot 03149$, and $\xi/E_\xi = 4 \cdot 279$. It seems rather questionable, therefore, whether our regression line can be regarded as strictly linear within the limits of the probable errors of random sampling.

The equation of the regression straight line is

$$y = 26 \cdot 517 + \cdot 772x.$$

I have plotted the means of arrays and find that they are scattered with some irregularity on either side of the theoretical line, but considering the nature of our material the fit is perhaps as good as could be expected. There is no suggestion in the graph that any other curve would give better results.

The biological significance of this constant showing the interdependence between length of flowering stalk and length of fruit seems to me to be chiefly descriptive. We cannot say that the length of the stalk determines to some extent the length of the fruit, or *vice versa*. In short, we have not "explained" anything, as we biologists are so wont to do. But we have measured a relationship and recorded it in a form that may be directly compared with other similar measurements when they shall have been made, and when the series so described are enough, comparison will permit of pushing problems further.

3. *The Relationship between the Length of the Peduncle and the Fertility of the Fruit.*

We have little definite knowledge of the relationship between the physical development of an organism and its fertility. In man Pearson * has brought forward evidence to show that tall women are more fertile than short ones. I am not aware of further published evidence for animals, though Professor Pearson informs me that data for other mammals are in existence and will shortly be published.

In plant material it has been shown in an earlier paper † that there is a correlation of about ·5 or over for length of flowering stalk and number of flowers per umbel in *Nothoscordium* and *Allium*. In the case of *Cercis* ‡ and *Celastrus* §, however, there seems to be no correlation between the size of the inflorescence as measured by the number of flowers or fruits produced and the fertility of the ovaries. Before we can go very far into the problem of the relationship between the degree of development of the vegetative and reproductive characters we must have together a sufficient number of series of reduced data to enable us to make extensive comparisons.

In the table of correlation constants I have given the relationship between the length of the peduncle and the fertility characters of "first" and "second"

* Pearson, K., *Proc. Roy. Soc.* Vol. LIX. pp. 301—305, 1896.

† Harris, J. Arthur, *Ann. Rept. Mo. Bot. Gard.* Vol. XX. 1909.

‡ Harris, J. Arthur, *Biometrika.* In press.

§ Harris, J. Arthur, *Ann. Rept. Mo. Bot. Gard.* Vol. XX. 1909.

locules separately. The constants were worked out largely to furnish an extra safeguard against arithmetical blunders, but the results seem worth publishing as this may indicate to the non-statistical reader something of the amount of divergence between constants which may arise purely as a result of chance.

The correlation coefficients and their probable errors are given in Table 4. In two cases I have also compared the correlation ratio, η. See Table 5. In these two cases Blakeman's formula cited above gives:—

For length and ovules, $\xi/E_\xi = 4{\cdot}125$,

For length and seeds, $\xi/E_\xi = 4{\cdot}131$.

Here again there is some question concerning the linearity of our regression. The equations to the regression lines are:—

For ovules per placenta, $y = 5{\cdot}863 + {\cdot}532x$,

For seeds per placenta, $y = 2{\cdot}987 + {\cdot}500x$.

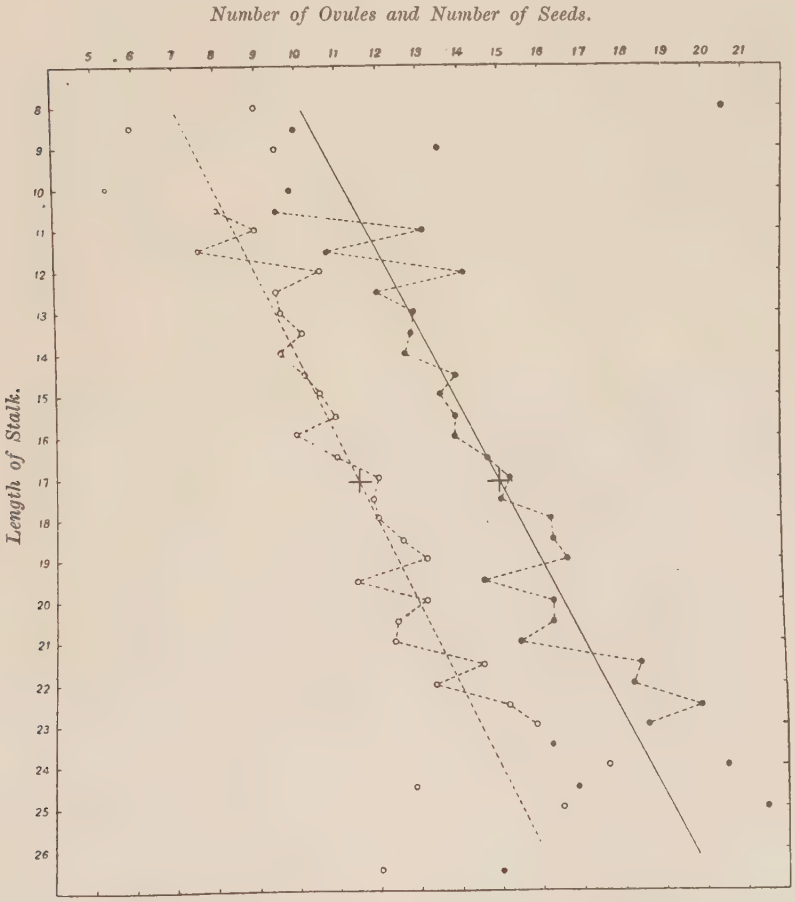


DIAGRAM II. Regression of number of ovules per placenta on peduncle length and of number of seeds per placenta on peduncle length. Solid dots and line = ovules; circles and dotted line = seeds. (Means depending on a few isolated observations not joined up.)

In neither case can the straight line be considered very satisfactory, but at present I do not see the advantage of seeking for further regression equations. When more biological data are available it may be profitable to go further with the statistical analysis.

TABLE 4.

Correlation between Length of Peduncle and the Fertility of the Fruit in Sanguinaria.

Relationship between Length of Stalk and	Correlation and Probable Error
Ovules on First Placenta	·313 ± ·019
Ovules on Second Placenta	·321 ± ·019
Ovules on First and on Second Placenta	·315 ± ·019
Total Ovules per Fruit	·323 ± ·019
Seeds on First Placenta	·341 ± ·019
Seeds on Second Placenta	·356 ± ·019
Seeds on First and on Second Placenta	·348 ± ·019
Total Seeds per Fruit	·363 ± ·019
Aborted Ovules on First Placenta	·039 ± ·021
Aborted Ovules on Second Placenta	·029 ± ·021
Aborted Ovules on First and on Second Placenta...	·034 ± ·021
Total Aborted Ovules per Fruit	·037 ± ·021

TABLE 5.

Relationship	r	η	$\eta - r$
Length and Ovules per Placenta ...	·315 ± ·019	·361 ± ·019	·046
Length and Seeds per Placenta ...	·348 ± ·019	·388 ± ·018	·040

These constants show that there is essentially the same degree of inter-dependence between the length of the fruit-stalk and the number of ovules formed and the number of seeds developing as between the length of the fruit-stalk and the length of the fruit. In the case of both ovules and seeds the correlation between length of peduncle and the total number per fruit is slightly, though *not significantly*, higher than for the numbers per placenta.

A question in which the biologist is interested is this:—Is the relationship between the length of the peduncle and the number of seeds developing more intimate than that between the length of the peduncle and the number of ovules formed?

Suppose for a moment that the degree of development of the peduncle affords some rough measure of the "vigor" of the plant. I think no physiologist can

object to it as a working hypothesis, and I dare say that most would take it for granted. Now it is important to know whether the vigor of the plant shows itself more in the number of seeds developing—a character which is not definitely and unchangeably fixed until after a considerable period of physiological activity—than in the number of ovules formed—a character definitely and unchangeably established relatively early in the development of the organ we are studying.

If vigor finds fuller expression in the number of seeds developing than in the number of ovules formed we should expect to find a higher coefficient of correlation between the length of stalk and the number of seeds than between the length of stalk and the number of ovules. The comparison is made in Table 6.

TABLE 6.

Comparison of Relationship between Stalk-Length and Number of Ovules and Stalk-Length and Number of Seeds.

Relation between Stalk-Length and	Correlation and Probable Error
Ovules on First and on Second Placenta ...	·315 ± ·019
Seeds on First and on Second Placenta ...	·348 ± ·019
Difference	+ ·033 ± ·027
Total Ovules per Fruit	·323 ± ·019
Total Seeds per Fruit	·363 ± ·019
Difference	+ ·040 ± ·027

It appears that while actually the correlation between length of stalk and number of seeds is slightly higher than that between length of stalk and number of ovules, the difference is so small as to be insignificant in comparison with its probable error.

4. *The Relationship between the Length of the Fruit and its Fertility.*

That there should be a fairly close interdependence between the size of a fruit like that of *Sanguinaria* and the number of ovules formed and the number of seeds developing is to be expected. The placental space in a fruit of this species is pretty well filled up with ovules, and if there are many ovules the placentae must be longer than if there are only a few. The demonstration by means of a "formidable mathematical apparatus" of this rather obvious fact is not in itself a matter of much weight, but it is of importance to measure the degree of resemblance instead of merely asserting that a relationship exists, and to express the result of the measurement in such terms that it may easily and directly be compared with like measures in other species.

The constants for both lots of data are compared in Table 7.

TABLE 7.

Correlation between Length of Fruit and Number of Ovules Formed and Seeds Developing per Placenta and per Fruit.

Relationship between Length of Fruit and	Correlation and Probable Error		
	1906	1907	Difference
Ovules on First and on Second Placenta	·517 ± ·016	·638 ± ·020	+ ·121 ± ·025
Total Ovules per Fruit	·527 ± ·015	·656 ± ·019	+ ·131 ± ·025
Seeds on First and on Second Placenta	·552 ± ·015	·661 ± ·019	+ ·109 ± ·024
Total Seeds per Fruit	·576 ± ·014	·688 ± ·018	+ ·112 ± ·023
Aborted Ovules on First and on Second Placenta	·078 ± ·021	·023 ± ·034	− ·106 ± ·040
Total Aborted Ovules per Fruit	·085 ± ·021	·025 ± ·034	− ·110 ± ·040

The equations to the regression straight lines for length of pod and ovules per placenta and for length of pod and seeds developing per placenta in the two series are given in Table 8. Table 9 affords a comparison of r and η for these relationships.

TABLE 8.

Equations to Regression Straight Lines for Fertility Characters on Pod Length.

Regression of Fertility Characters on Pod Length	Equation to Straight Line
Ovules per Placenta on Pod Length, 1906 ...	$y = \cdot 063 + \cdot 378x$
Seeds per Placenta on Pod Length, 1906 ...	$y = -2 \cdot 115 + \cdot 344x$
Ovules per Placenta on Pod Length, 1907 ...	$y = -4 \cdot 000 + \cdot 324x$
Seeds per Placenta on Pod Length, 1907 ...	$y = -7 \cdot 438 + \cdot 332x$

TABLE 9.

Comparison of r and η and Blakeman's Test for the Linearity of Regression for Fruit Length and Fertility Characters.

Relationship	Coefficient of Correlation, r	Correlation ratio, η	$\eta^2 - r^2 = \xi$	ξ / E_ξ
Ovules and Length, 1906	·517 ± ·016	·532 ± ·015	·016	2·971
Seeds and Length, 1906	·552 ± ·015	·552 ± ·015	—	—
Ovules and Length, 1907	·638 ± ·020	·637 ± ·020	—	—
Seeds and Length, 1907	·661 ± ·019	·677 ± ·018	·021	2·160

In both of the cases in which ξ/E_ξ was calculated it is found to have a value not greatly exceeding 2·5 in the first case and falling somewhat short in the second. Judging merely from the statistics in hand, the deviations of the means of arrays from a straight line may be due to nothing more than the probable errors of random sampling. In the case of length and seeds for 1906 and length and ovules for 1907, ξ/E_ξ was not calculated, since η is slightly less than r. This theoretically impossible result is explained as follows. In calculating η I followed the conventional practice of using the standard deviation for the population calculated without Sheppard's modification. In calculating r, however, Sheppard's correction was applied to both standard deviations. Had standard deviations *without* Sheppard's correction been used, r would have been lowered somewhat by the increase in the value of the product $\sigma_1\sigma_2$, and would then be less than η. It seems rather pedantic under the circumstances to recalculate the values of r, for biologically the results could have little increased value. For one year it is the correlation for length and ovules and for the other year it is the correlation for length and seeds which give such nearly identical values of r and η. There is, therefore, no reason to think that the regression may be non-linear for ovules and not for seeds, or *vice versa*.

For both ovules and seeds the constants for 1907 are about $0{\cdot}110 \pm {\cdot}025$ higher than in 1906.

Possibly the more mature condition of the fruits gathered in 1907 may have as one of its results the raising of the coefficient of correlation for length of fruit and number of ovules or seeds.

These constants are purely descriptive. They express the degree of interdependence between length of fruit and number of ovules or seeds but do not explain anything concerning the cause of this interdependence. The correlation coefficients in themselves cannot even be considered adequate to establish the existence of a causal nexus inherent in the fruit primordium and finding expression in the relationship observed between length of fruit and number of ovules or seeds. There are two ways in which a correlation between fruit length and fertility might arise independently of any cause for such a relationship innate in the fruit.

(*a*) Heterogeneity in the individuals from which the fruits were taken.

(*b*) Correlation between both the length and the fertility of the fruit and some other organ of the plant, say length of peduncle.

The first of these possible sources of correlation is not open to investigation on the basis of our material. The second may more easily be taken into account.

Both length of fruit and number of ovules and seeds are correlated with length of peduncle. What influence will this have upon the correlation between the length of the fruit and its fertility? The best way of getting at this problem seems to be to determine the correlation between the length of the pod and its fertility

characters for constant length of stem. In doing this we strip off, so to speak, the influence of the length of the stalk upon the coefficient of correlation between length and fertility of fruit and have our relationship uninfluenced by this character.

The familiar formula for the partial correlation coefficient is

$$\rho_{xy} = \frac{r_{xy} - r_{xz}\, r_{yz}}{\sqrt{1 - r_{xz}^2}\,\sqrt{1 - r_{yz}^2}}.$$

In the present series x is length of fruit, y the number of ovules or seeds, as the case may be, and z, whose influence on the gross correlation is to be eliminated, is the length of the peduncle.

The partial correlation coefficients for the total ovules and total seeds for the 1906 series calculated by the above formula appear in Table 10.

TABLE 10.

Correlation between Length of Fruit and Number of Ovules and Seeds per Fruit for Variable and Constant Lengths of Peduncle.

Relationship between Length of Fruit and	Correlation
Total Ovules per Pod	
For Variable Length of Peduncle ...	·527
For Constant Length of Peduncle ...	·470
Difference	·057
Total Seeds per Pod	
For Variable Length of Peduncle ...	·576
For Constant Length of Peduncle ...	·517
Difference	·059

The differences show that there is a small amount of the relationship between the length of the fruit and the number of ovules or seeds which it produces which is due to the correlation of both of the characters of the associated pair with peduncle length. But this influence is comparatively small.

A question of very real interest is that concerning the relative magnitude of the correlation between the length of the fruit and the number of ovules and the length of the fruit and the number of seeds. The results may be extracted from Table 7 for comparison in Table 11.

While the correlation for length and seeds is the higher in both series, the excess above the relationship for length and ovules cannot be given much weight. I was at first somewhat surprised at this result, for I had rather expected that the length of a soft fruit of this kind was much more dependent upon the number of

seeds developing than upon the number of ovules formed. We are dealing, however, with a form in which the correlation between the number of ovules formed

TABLE 11.

Comparison of Correlation for Fruit Length and Number of Ovules and Fruit Length and Number of Seeds.

Relationship	Correlation in 1906	Correlation in 1907
Length and Ovules	·527 ± ·015	·656 ± ·019
Length and Seeds	·576 ± ·014	·688 ± ·017
Difference ...	·049 ± ·021	·032 ± ·026

and the number of seeds developing is high. Under such circumstances these results would be expected. The problem should be worked out on some form in which there is only a moderate correlation between the number of ovules and number of seeds.

5. *The Relationship between the Number of Ovules Formed and the Number of Seeds Developing.*

Our knowledge of the physiology of seed production is very limited. We know practically nothing of why one ovule in a locule develops into a mature seed and another beside it fails to develop. General explanations, such as "lack of nutrition," "failure to be fertilized," etc., are thoroughly unsatisfactory. Experimental evidence would be the most desirable, but until it is forthcoming I think we shall have to depend upon the biometric analysis of large bodies of data to enable us to get some insight into the problems.

TABLE 12.

Correlation for Fertility Characters in Sanguinaria.

Relationship between Number of Ovules Formed and Number of Seeds Developing	Correlation and Probable Error		
	1906	1907	Difference
For First Placenta	·7901 ± ·0080	·7998 ± ·0121	—
For Second Placenta	·8037 ± ·0076	·7905 ± ·0127	—
For First and Second Placenta ...	·7970 ± ·0055	·7951 ± ·0088	− ·0019 ± ·0104
For the Fruit	·8141 ± ·0072	·8106 ± ·0116	− ·0035 ± ·0136

The first question which suggests itself for investigation is whether an ovule formed in a pod producing a large number of ovules is more, or less, likely to

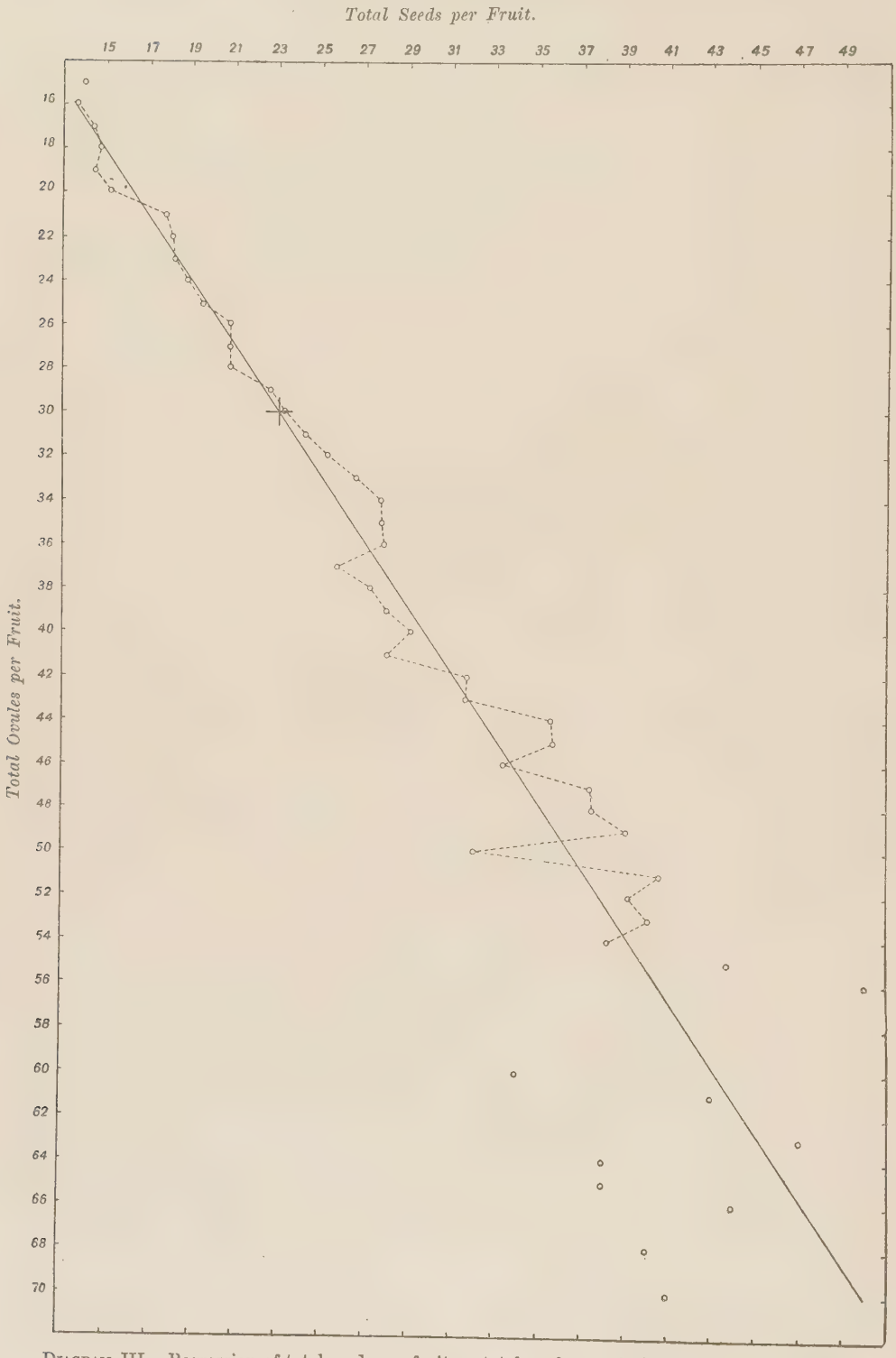


Diagram III. Regression of total seeds per fruit on total ovules per fruit. (Means depending on a few isolated observations not joined up.)

develop than one which is produced in a fruit with a smaller number of ovules. This will "explain" nothing when we arrive at the answer to this question. If we find that it has a better chance of developing in a fruit of some particular type we shall not know *why* it has this advantage over an ovule formed in a different type of pod. But we shall have definitely isolated one of the factors—or complexes of factors—which have an influence upon the development of an ovule into a seed and shall have quantitatively determined the amount of influence which it wields.

The ordinary coefficients of correlation between the number of ovules and the number of seeds developing per pod are shown in Table 12.

Two points in this table are noteworthy. First, the correlations are high. Second, the agreement between the two series of material is very close. The constants for the two collections differ by less than the probable error of their difference.

The equations to the regression straight lines for total ovules and total seeds per fruit are:—

$$\text{For 1906 series,} \quad y = 2{\cdot}808 + {\cdot}677x,$$

$$\text{For 1907 series,} \quad y = -{\cdot}741 + {\cdot}719x.$$

The comparison of η with the coefficients of correlation and the ξ/E_ξ of Blakeman's test for the linearity of regression are given in Table 13.

TABLE 13.

Tests for Linearity of Regression in Correlation between Total Ovules and Total Seeds.

Constant	1906 Series	1907 Series
Coefficient of Correlation, r ...	·8141 ± ·0072	·8106 ± ·0116
Correlation Ratio, η	·8317 ± ·0066	·8420 ± ·0098
ξ, $=\eta^2 - r^2$	·02896	·05199
Blakeman's Criterion, ξ/E_ξ ...	4·028	3·438

I am rather surprised to find ξ/E_ξ so high as it is in these two cases. The indications are that the divergence of the means from a straight line is rather larger than one would expect to be accounted for by the errors of sampling. From a graph, however, there is no clear indication that any curve would give any better fit than the straight line. Certainly the data in hand do not justify any further analysis along these lines. The two constants are so nearly identical that for any practical purpose it is immaterial which is employed to describe the relationship between number of ovules and number of seeds per fruit.

While the coefficient of correlation shows the degree of interdependence of number of ovules and seeds it does not yield one other item of information which

is essential to our understanding of fertility. This is a knowledge of the influence of the number of ovules per fruit upon the chance of an individual ovule developing into a seed. This is quite a different problem from that of the relationship between the total number of ovules per pod and the total number of seeds developing.

As pointed out in an earlier paper*, the constant which we need is the correlation between the number of ovules and the deviation of the number of seeds from their probable number. The coefficients calculated by the method described in that paper are given in Table 14.

TABLE 14.

Correlation between Number of Ovules and Deviation of Number of Seeds from the Probable Number on the Assumption of the same Proportional Fecundity throughout the Population.

Relationship between Ovules and Seeds	Correlation and Probable Error		
	1906	1907	Difference
For both Placentae ...	$-\cdot178\pm\cdot015$	$\cdot038\pm\cdot024$	$+\cdot216\pm\cdot028$
For the Fruit	$-\cdot191\pm\cdot021$	$\cdot051\pm\cdot034$	$+\cdot241\pm\cdot039$

These results show how important it is to have more than one series of material upon which to base conclusions when the correlations are of a low order of magnitude. Considering only the 1906 series we see constants with a negative sign and about ten times their probable error. It would seem from these results alone that as the number of ovules per placenta or per fruit increased the capacity of the fruit for maturing its ovules into seeds decreased. But when the 1907 series is taken into consideration also we note that the correlations are hardly twice their probable errors but with the positive sign. Of course it may be possible that a larger sampling in the 1907 plants would have given a sensible negative correlation. But all that we can conclude is that while there may be a relationship between the number of ovules per fruit and the capacity of the fruit for maturing its seeds there is no very satisfactory argument for such a theory in our present collections.

These conclusions agree with the results announced for one sample of 10,000 pods of *Cercis* given as an illustration in the paper on the method of determining the relationship between ovules and seeds, but do not agree with the results for the general sample of *Robinia*. The factors underlying differences of this kind deserve investigation.

* Harris, J. Arthur, *Biometrika*, Vol. VI. pp. 438—443, 1909.

6. *The Correlation between the two Placentae of the Fruit.*

The correlations between organs of the two sides are now available for a considerable number of bilaterally symmetrical animal organisms but practically nothing has as yet been put into print concerning the similarity of the two sides of bilaterally symmetrical plant organs. The fruit of *Sanguinaria* furnishes a very suitable object for the determination of such relationships.

Since the two placentae are quite undifferentiated the correlations may be determined from symmetrical tables. Our tables as published have not been rendered symmetrical. Anyone who desires may determine the constants from them as they stand. I have worked by both methods and find no sensible difference in results. The coefficients for symmetrical tables can easily be calculated from them by use of proper formulae*. For the constants see Table 15.

TABLE 15.

Organic Correlations for Fruits of Sanguinaria.

Characters of the two Placentae	Correlation and Probable Error		
	1906	1907	Difference
Ovules of "First" Placenta and Ovules of "Second" Placenta	·9239 ± ·0031	·8868 ± ·0072	− ·0371 ± ·0078
Seeds of "First" Placenta and Seeds of "Second" Placenta	·8014 ± ·0076	·8443 ± ·0097	+ ·0429 ± ·0123
Aborted Ovules on "First" Placenta and Aborted Ovules on "Second" Placenta	·6793 ± ·0115	·7070 ± ·0169	+ ·0277 ± ·0204
Ovules on "First" Placenta and Seeds Developing on "Second" Placenta	·7356 ± ·0098	·7191 ± ·0163	− ·0165 ± ·0193

The equation of the regression straight line for the relationship for ovules is

$$y = 1{\cdot}1384 + {\cdot}9239\,x.$$

A graphic representation of the slope and the empirical means shows that except at the ends, where the observations are few, the observed and the theoretical means show a close agreement. We also note:—

Ovules of first and ovules of second placenta,

$$r = \cdot9239 \pm \cdot0031,$$
$$\eta = \cdot9229 \pm \cdot0032,$$
$$\eta - r = -\cdot0010.$$

Seeds of first and seeds of second placenta,

$$r = \cdot8014 \pm \cdot0076,$$
$$\eta = \cdot8015 \pm \cdot0076,$$
$$\eta - r = \cdot0001.$$

* See R. Pearl, *Biometrika*, Vol. v. pp. 250, 251.

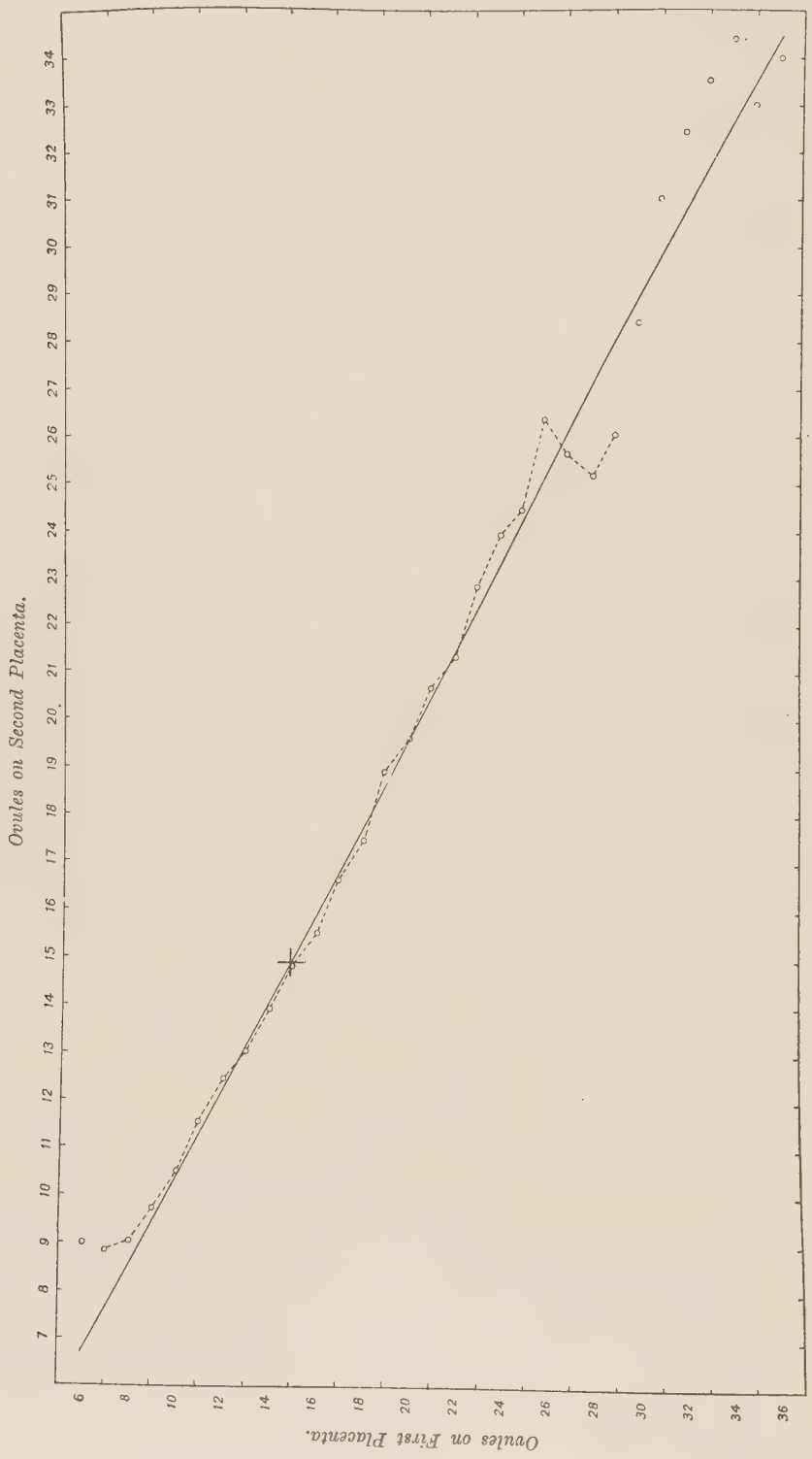


Diagram IV. Regression of number of ovules on "second" placenta on number of ovules on "first" placenta.
(Means depending on a few isolated observations not joined up.)

The agreement is remarkably close. The condition $r > \eta$ for the ovules is due to the use of Sheppard's correction in the calculation of r. Without Sheppard's correction for the standard deviations we find $r = \cdot 9203$, where the theoretical condition $\eta > r$ is fulfilled. For the direct correlation for both ovules and seeds developing the differences in the constants for the two seasons are about four times their probable errors. But considering the possibilities of error in the sampling of the habitat in the two years I would not attach much significance to them as indicating real biological differences in the two years. The other two correlations differ in the two years by an amount of about the magnitude of its probable error.

The significance of these constants seems to me to be twofold: (a) descriptive, (b) as a means of gaining a somewhat deeper insight into the physiology of seed development.

The constant showing the degree of interdependence between the number of ovules on the two placentae taken in connection with the means and standard deviations, is a terse description of these features in the fruits of a given population. As it stands it is interesting as showing the high degree of perfection in the morphogenetic process giving rise to the ovary*. After studying a considerable mass of reduced data, some of which I hope to publish before long, it seems to me that a comparative study of these interplacental or interlocular correlation coefficients presents some problems of considerable interest.

These organic correlation coefficients seem to me to illustrate very well the way in which statistical methods may be applied to certain physiological problems.

The direct correlation for number of ovules per placenta is higher than that for either number of seeds developing or number of abortive ovules. Since the ovules are uninfluenced by the accidents of nutrition or fertilization which affect the number of seeds or the number of aborted ovules, the correlation between the number of ovules on the two placentae is quite independent of that for the other fertility characters; but the converse is not at all true.

We may ask to what extent the correlation between the number of ovules per placenta influences the correlation for number of seeds per placenta. The number of seeds developing per placenta is closely correlated with the number of ovules, hence a correlation between the number of seeds on the first and on the second placenta would necessarily be found even though there were no *physiological* relationship whatever between the development of the seed on the two sides of the fruit. Our object now is to remove the influence of the correlation for number of ovules on first and on second placenta from the correlation for seeds developing. If there is a physiological interdependence between the two placentae of the fruit depending on some factor other than the number of ovules present, we should expect to find a significant correlation between the number of seeds developing after the contribution of the ovules has been removed.

* Since the tables are symmetrical the slope of the regression line is the same as the coefficient of correlation.

Again we have recourse to the partial correlation coefficient as in the investigation of the influence of peduncle length upon the relationship between the length of the fruit and its fertility characters. Here the more complex formula for four variables must be used.

The partial correlation, $\mathbf{r}_{s_1 s_2}$, for constant values of o_1 and o_2 is given by the formula*

$$\mathbf{r}_{s_1 s_2} = \frac{r_{s_1 s_2}(1 - r_{o_1 o_2}{}^2) - r_{o_1 s_1} r_{o_1 s_2} - r_{o_2 s_1} r_{o_2 s_2} + r_{o_1 o_2}(r_{o_1 s_1} r_{o_2 s_2} + r_{o_1 s_2} r_{o_2 s_1})}{\sqrt{(1 - r_{o_1 o_2}{}^2 - r_{o_2 s_1}{}^2 - r_{o_1 s_1}{}^2 + 2 r_{o_1 o_2} r_{o_1 s_1} r_{o_2 s_1})}\sqrt{(1 - r_{o_1 o_2}{}^2 - r_{o_2 s_2}{}^2 - r_{o_1 s_2}{}^2 + 2 r_{o_1 o_2} r_{o_1 s_2} r_{o_2 s_2})}},$$

where o_1 and o_2, s_1 and s_2 are the ovules and seeds on the first and second placenta. But since our tables have been rendered symmetrical $o_1 = o_2$, $s_1 = s_2$ and we may write

$$\mathbf{r}_{g'g'} = \frac{r_{g'g'}(1 - r_{o'o'}{}^2) - 2 r_{og} r_{o'g'} + r_{o'o'}(r_{og}{}^2 + r_{o'g'}{}^2)}{1 - r_{o'o'}{}^2 - r_{o'g'}{}^2 - r_{og}{}^2 + 2 r_{o'o'} r_{og} r_{o'g'}},$$

where the correlations without dashes are those for the same placenta and those with dashes are for the relationships between different placentae.

Substituting constants for these formulae we find :—

For 1906, $\mathbf{r}_{g'g'} = \cdot 590$,

For 1907, $\mathbf{r}_{g'g'} = \cdot 714$.

Remembering that our gross correlations were $r = \cdot 801$ for 1906 and $r = \cdot 844$ for 1907 it is clear that :—

(*a*) A certain amount of the correlation between the number of seeds developing is due to the correlation between the number of ovules formed on the two placentae.

(*b*) The correlation is mainly due, however, to the physiological factors upon which the development of ovules into seeds depends.

Under the term physiological as used here are comprehended, (*a*) the ecological factors which determine whether an ovule shall receive a sperm, and the availability of food material and other requisites for growth, (*b*) the innate vigor or other physiological characters of the individual which determine whether a fertilized ovule shall develop into a seed.

The foregoing analysis shows, I believe, that the main bulk of the correlation between the number of seeds on the two placentae is due to a complex of ecological and physiological causes, and is not merely a necessary statistical result of the correlation for ovules. Some of the biological factors may have an influence on both the number of ovules formed and the proportion of these ovules which shall develop into seeds. Experimental data in quantity and form for statistical treatment are desirable.

* Pearson, K., *Phil. Trans.* A, Vol. cc. p. 31, 1902.

IV. Recapitulation.

The present paper embodies the results of a quantitative study of the indorescence and fruit of *Sanguinaria* with special reference to morphological and physiological problems. The results presented are chiefly of value as descriptive quantitative data. The constants will gain in significance as others from organs of distinct morphological type are laid beside them for comparison. The following points may be noted.

1. The variation and correlation constants for two collections of *Sanguinaria* made in the same habitat for two successive years show very close agreement for some characters and significant divergence for others. The significant differences do not necessarily indicate that the populations in the two years are really different for the material was not collected in a manner to secure the same proportional sampling of subhabitats in the two seasons, and these may differ enough among themselves to introduce a slight heterogeneity into the material. These results furnish another illustration of the difficulty of establishing "place constants" for an organism easily affected by environment. On the whole, the two series may be regarded as very satisfactorily similar as material for a morphological and physiological investigation.

2. The length of the peduncle and the length of the fruit are to some extent interdependent, the magnitude of the relationship being measured by a coefficient of correlation of about ·350. Number of ovules formed, number of seeds developing and number of aborted ovules per fruit are also correlated with peduncle length. For both ovules and seeds developing the correlation is about ·300—·350, the intensity of the relationship being slightly but not significantly higher for length of fruiting stalk and number of seeds developing than for length of fruiting stalk and number of ovules formed.

Since our correlation coefficient is purely descriptive, we cannot say that the length of the peduncle has any influence upon the length of the fruit or upon its fertility. All that we can do at present is to measure the degree of interdependence of these pairs of characters. Possibly neither has any real influence upon the development of the other and their measurable interdependence is due merely to the age or physiological vigor of the plants which produced them. Interdependence of their magnitude would then be merely the result of their mutual dependence upon other factors. Perhaps questions of this sort can be solved only by appropriate experiments, but we sorely need quantitative data of the kind presented here for numerous species to guide us in the intelligent planning of experiments.

3. It is not surprising to find that length of fruit and number of ovules formed and length of fruit and number of seeds developing are interdependent, but it is important to measure the degree of correlation between them in such a way that comparison with other forms will be possible. As indicated by our material the correlations for length and ovules or seeds developing lie between ·500 and ·700.

The correlation between the length of fruiting stalk and both length of fruit and fertility characters of the fruit would necessarily bring about a statistical correlation between length of the fruit and its fertility characters, whether there is any other biological reason for such a correlation or not. Calculation for the relationship between length of fruit and its fertility for constant stem length shows that the removal of the influence of this latter character reduces the correlation but slightly.

Clearly, then, the correlation between length and fertility of fruit is due mainly to causes other than the length of peduncle.

In both series the correlation between the length of the fruit and the number of seeds developing is slightly but hardly significantly higher than that between the length of the fruit and the number of ovules formed.

4. The correlation between the number of ovules formed and the number of seeds developing per placenta or per fruit is high, the constants falling immediately above or below ·800. This is obviously a necessary condition when the ratio seeds/ovules is high, as it is in this case, and the quantitative statement of the degree of the relationship is chiefly of interest in comparison with similar constants from other material. It tells us nothing concerning the influence of the number of ovules formed per fruit upon the probability of an individual ovule developing into a seed. In other words, the coefficient of correlation between number of ovules formed and number of seeds developing is a measure of the influence of the number of ovules upon the *absolute* fertility as measured by the number of seeds developing, but not upon *relative* fertility.

A suitable constant for the measurement of this latter relationship indicates that in the 1906 series as the number of ovules per fruit increases the capacity of the fruit for maturing its ovules into seeds decreases slightly, while in the 1907 collection there seems to be no significant influence of the number of ovules per fruit upon the chances of an ovule developing into a seed.

It has been suggested to me that the fruits in which the larger number of ovules are formed are the more vigorous, and that in consequence they will be able to develop a higher per cent. of their ovules into seeds. The discordant results secured in this study as well as those announced earlier for *Cercis* and *Robinia* indicate that extreme caution must be used in asserting that the number of ovules formed in a fruit has anything to do with the proportion of them which shall develop into seeds.

5. The correlations between the two placentae are all high, the direct correlation for the number of aborted ovules being actually the lowest, although the cross correlation for ovules of the "first" placenta and seeds developing on the "second" placenta is perhaps not significantly higher. The values for the direct correlations for seeds developing are ·80 and ·84 and those for ovules ·89 and ·92 in the two series. These constants show that there is a high degree of bilateral symmetry in the fruit of *Sanguinaria*.

Since the number of ovules formed and the number of seeds developing on a placenta are correlated, the correlation between the number of ovules per placenta must bring about a correlation between the number of seeds developing on the two placentae. Evidence is adduced to show that the correlation for seeds is not entirely due to the correlation for ovules, but that the larger part of the inter-relationship for the seeds developing must be due to ecological or physiological factors.

London, December 7, 1909.

V. Appendix of Correlation Tables of Data.

The tables for the relationships between the two placentae are not rendered symmetrical except for the cross correlation for ovules of "first" placenta and seeds of "second" placenta where it is obviously necessary to use each placenta once as a "first" and once as a "second" placenta or publish two tables.

It is unnecessary to publish tables for the following four relationships:—length of peduncle and total ovules, length of peduncle and total seeds, length of fruit and total ovules, length of fruit and total seeds. The product moments for these can be easily derived from those for ovules and seeds per placenta by taking the origin at 0 in calculating the first moments of the arrays of number of ovules or number of seeds per placenta. Since both placentae of a fruit were entered under the same grade of peduncle length or fruit length the first moment for the array gives at once the total number of ovules or seeds formed by that grade of peduncle or fruit. This is the same for the number per placenta or the total number per fruit. The origin of the other character—peduncle length or fruit length in the present case—may then be taken wherever we please, and the product moment calculated at once by multiplying up the first moments of the arrays by the deviations of the classes of the other character from their origin. The means of arrays for tests of linearity of regression may be easily obtained. The standard deviations for total ovules and total seeds per fruit are available from other tables.

The correlation tables for number of ovules failing per placenta and per fruit are also omitted, for if their standard deviations be known the more important correlations can be derived from the tables for ovules formed and seeds developing.

TABLE I., 1906.

Length of Peduncle and Length of Fruit.

Length of Peduncle.

Length of Fruit.	8	8·5	9	9·5	10	10·5	11	11·5	12	12·5	13	13·5	14	14·5	15	15·5	16	16·5	17	17·5	18	18·5	19	19·5	20	20·5	21	21·5	22	22·5	23	23·5	24	24·5	25	25·5	26	26·5	Totals
21—23	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
24—26	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	1	–	1	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	4
27—29	–	–	–	–	3	–	–	–	–	1	1	1	3	2	–	–	7	3	2	1	–	1	–	–	–	1	1	1	–	–	–	–	–	–	–	–	–	–	28
30—32		1	–	–	–	2	3	3	2	3	2	6	5	6	6	3	9	8	3	5	3	3	4	–	–	3	1	–	–	–	–	–	–	–	–	–	–	–	81
33—35	–	–	–	–	–	3	–	4	–	5	4	6	8	13	15	13	13	17	17	7	7	9	4	5	6	5	2	1	–	–	1	1	1	1	–	–	–	–	168
36—38	–	–	1	–	–	1	4	–	4	4	4	6	7	14	10	9	18	9	16	12	17	9	9	4	8	4	5	3	1	–	2	–	–	2	–	–	–	–	183
39—41	–	–	–	–	–	–	–	–	3	2	6	5	5	4	4	14	17	13	9	10	11	12	11	7	5	5	4	1	4	3	2	–	–	–	1	–	–	1	159
42—44	–	–	–	–	–	–	–	1	2	–	2	2	4	3	7	8	11	10	12	14	12	16	19	7	9	6	4	–	–	2	1	1	1	–	–	–	–	–	154
45—47	–	–	–	–	–	–	1	–	–	1	3	2	–	3	6	7	6	10	4	6	7	8	12	5	4	5	3	3	–	3	5	–	–	–	–	–	–	–	104
48—50	1	–	–	–	–	–	–	1	–	–	–	–	1	1	2	2	3	2	5	8	7	6	6	3	7	7	3	1	2	1	1	–	–	1	–	–	–	–	71
51—53	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	1	4	1	1	1	2	6	–	1	–	2	–	1	1	2	–	1	–	–	–	25
54—56	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	1	–	1	1	–	–	1	2	1	–	–	–	1	–	–	–	–	–	–	–	–	–	–	10
57—59	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	1	1	1	–	2	–	–	1	–	–	–	–	–	1	–	–	–	–	–	–	–	9
60—62	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	–	–	–	–	–	–	2
63—65	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
Totals	1	1	1	0	3	6	8	9	13	16	22	28	33	47	51	59	85	75	71	69	67	68	68	34	47	36	23	12	9	9	14	4	4	4	2	0	0	1	1000

TABLE II., 1906.

Length of Peduncle and Number of Ovules on First and on Second Placenta.

Length of Peduncle.

Ovules on First Placenta and on Second Placenta.	8	8·5	9	9·5	10	10·5	11	11·5	12	12·5	13	13·5	14	14·5	15	15·5	16	16·5	17	17·5	18.	18·5	19	19·5	20	20·5	21	21·5	22	22·5	23	23·5	24	24·5	25	25·5	26	26·5	Totals
6	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
7	–	–	–	–	–	1	–	2	–	–	–	–	1	1	–	–	–	1	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	8
8	–	–	–	–	1	2	1	1	–	2	–	1	1	2	3	2	6	2	–	3	1	1	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	30
9	–	–	–	–	3	5	–	1	3	4	4	7	5	3	5	10	8	5	3	4	3	5	3	3	–	–	–	2	–	–	–	–	–	–	–	–	–	86
10	–	2	–	–	–	2	2	4	2	6	8	11	11	8	8	12	13	15	12	7	5	3	2	4	3	5	4	3	1	–	1	–	–	1	–	–	–	–	155
11	–	–	–	–	1	–	2	5	1	6	8	9	7	15	19	11	28	11	12	11	9	8	8	11	5	7	4	–	2	–	–	–	1	1	–	–	–	–	202
12	–	–	–	–	–	–	1	2	7	3	2	8	10	14	14	15	20	26	15	19	11	9	11	8	10	5	7	1	1	–	3	2	1	1	–	–	–	–	226
13	–	–	1	–	1	2	4	1	1	4	5	6	10	14	11	16	20	19	9	14	15	14	12	7	8	9	7	1	–	2	2	1	–	–	–	–	–	–	216
14	–	–	1	–	–	–	2	1	1	–	5	1	5	6	8	12	15	11	14	23	13	12	13	5	3	5	3	1	–	–	–	1	–	1	–	–	–	–	162
15	–	–	–	–	–	–	1	–	1	–	3	2	4	3	7	11	16	12	21	8	14	12	16	4	13	5	4	–	–	–	2	2	–	–	–	–	–	2	163
16	–	–	–	–	–	–	1	–	1	3	3	4	5	5	10	9	10	6	6	6	6	15	14	7	12	5	4	1	1	1	3	–	–	–	–	–	–	–	138
17	–	–	–	–	–	–	–	1	2	2	1	2	1	2	6	1	5	8	9	6	12	9	16	3	12	6	2	4	2	1	2	–	–	–	–	–	–	–	115
18	–	–	–	–	–	–	1	–	3	1	1	1	3	5	2	2	11	7	7	7	17	8	4	2	4	3	1	2	1	4	1	–	–	2	–	–	–	–	100
19	1	–	–	–	–	–	1	–	2	1	3	–	1	6	1	2	1	4	14	6	4	11	5	6	6	5	1	3	3	2	1	–	–	–	–	–	–	–	90
20	–	–	–	–	–	–	–	–	1	–	1	2	–	5	4	3	6	4	5	3	5	8	6	2	1	3	1	3	3	4	3	–	–	2	–	–	–	–	72
21	–	–	–	–	–	–	–	–	1	–	–	–	2	1	–	3	2	4	6	5	2	5	3	3	4	4	–	–	1	1	1	–	1	–	–	–	–	–	49
22	1	–	–	–	–	–	–	–	–	–	–	–	–	2	–	2	6	3	3	3	6	3	8	1	2	2	1	–	1	1	2	–	1	–	1	–	–	–	49
23	–	–	–	–	–	–	–	–	–	–	–	–	–	1	2	2	–	6	3	3	2	2	5	1	3	–	2	–	–	–	–	–	1	–	–	–	–	–	33
24	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	2	–	1	3	1	–	3	1	–	2	2	1	–	–	–	2	1	2	–	1	–	–	–	23
25	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	2	–	2	–	2	1	1	1	1	2	2	1	–	–	–	3	1	–	2	–	–	–	–	20
26	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	1	2	2	2	–	–	1	1	2	1	–	2	–	–	–	–	–	–	17
27	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	3	–	1	1	2	1	–	–	–	–	1	–	–	–	–	–	10
28	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	–	1	–	1	1	1	1	–	1	2	–	1	1	–	–	–	–	–	–	–	–	–	12
29	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
30	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	2	1	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	5
31	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	2
32	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	2	1	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	5
33	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	2
34	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	2	–	–	–	–	1	–	1	–	–	–	–	–	–	–	–	5
35	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
36	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	2
Totals	2	2	2	0	6	12	16	18	26	32	44	56	66	94	102	118	170	150	142	138	134	136	136	68	94	72	46	24	18	18	28	8	8	8	4	0	0	2	2000

TABLE III., 1906.

Length of Peduncle and Number of Seeds Developing on First and on Second Placenta.

Length of Peduncle.

Seeds Developing on First and on Second Placenta.	8	8·5	9	9·5	10	10·5	11	11·5	12	12·5	13	13·5	14	14·5	15	15·5	16	16·5	17	17·5	18	18·5	19	19·5	20	20·5	21	21·5	22	22·5	23	23·5	24	24·5	25	25·5	26	26·5	Totals
0	–	–	–	–	–	–	–	–	–	–	–	—	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	1
1	–	–	–	–	–	–	–	–	–	–	–	—	–	–	–	–	1	–	–	–	1	1	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	4
2	–	–	–	–	–	–	–	–	–	–	–	—	1	–	–	1	–	–	–	–	–	1	1	–	–	2	–	–	–	–	–	–	–	–	–	–	–	6
3	–	–	–	–	1	–	–	–	–	–	–	—	1	–	–	1	1	2	–	1	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	9
4	–	–	–	–	1	–	2	2	1	–	–	—	1	2	1	–	2	–	–	1	2	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	16
5	–	–	–	–	2	1	–	–	–	1	–	—	1	3	3	–	6	9	–	–	1	1	–	–	–	1	–	1	–	–	–	1	–	–	–	–	–	–	31
6	–	2	–	–	1	3	–	3	3	2	5	2	4	4	5	4	9	5	3	6	5	3	1	2	–	1	–	2	–	–	–	1	–	–	–	–	–	–	76
7	–	–	–	–	–	1	2	5	1	5	7	8	6	10	8	5	15	9	7	7	5	5	3	3	1	–	1	1	–	–	–	–	–	–	–	–	–	–	115
8	–	–	–	–	–	4	1	3	2	5	6	5	6	12	10	17	14	14	7	13	7	3	8	2	4	3	3	1	–	–	–	–	–	–	–	–	–	–	150
9	2	–	1	–	1	1	4	1	2	2	9	11	13	13	15	18	21	17	19	10	9	14	11	11	12	6	6	–	2	1	1	–	1	2	–	–	–	–	236
10	–	–	1	–	–	–	2	1	4	7	4	11	16	14	14	15	31	15	17	16	16	15	14	7	5	8	6	1	1	2	–	–	1	2	–	–	–	–	246
11	–	–	–	–	–	–	3	3	4	2	2	7	2	9	10	14	31	15	14	19	15	11	10	13	15	7	5	–	3	–	2	4	–	–	–	–	–	1	221
12	–	–	–	–	–	–	1	–	3	3	1	1	3	5	11	12	11	18	16	14	14	17	21	9	10	7	8	2	3	1	2	–	–	1	2	–	–	–	196
13	–	–	–	–	–	2	1	–	–	4	5	4	5	6	7	8	9	11	17	9	15	17	8	4	7	7	5	1	1	2	5	–	–	1	–	–	–	1	162
14	–	–	–	–	–	–	–	–	3	1	2	3	2	6	10	5	5	10	14	9	10	11	11	2	6	5	4	2	2	2	–	–	–	–	–	–	–	–	125
15	–	–	–	–	–	–	–	–	–	–	2	–	4	2	3	6	4	4	6	10	10	9	14	6	6	7	–	5	2	2	3	–	–	–	–	–	–	–	105
16	–	–	–	–	–	–	–	–	1	–	–	2	1	–	–	1	4	7	7	6	10	4	9	2	8	3	1	2	1	–	2	–	–	–	–	–	–	–	71
17	–	–	–	–	–	–	–	–	2	–	1	–	–	5	1	3	1	7	5	3	5	3	7	2	3	5	1	1	–	3	4	–	–	–	–	–	–	–	62
18	–	–	–	–	–	–	–	–	–	–	–	1	–	2	1	2	2	2	5	4	2	6	7	2	8	1	–	–	1	3	3	–	2	1	–	–	–	–	55
19	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	3	2	2	3	5	6	5	3	–	3	1	2	–	1	–	3	–	1	–	–	–	–	–	42
20	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	2	1	2	–	2	1	1	2	1	2	2	2	1	1	–	–	–	1	–	1	–	–	–	23
21	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	—	–	3	1	1	–	2	2	–	–	–	–	1	–	–	–	–	–	–	12
22	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	2	–	3	–	–	1	1	–	–	–	–	2	1	–	1	1	1	–	–	15
23	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	—	–	–	–	–	1	1	–	1	–	–	–	1	1	–	–	–	–	–	5
24	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1	–	–	–	1	–	–	4	–	1	–	–	1	–	–	1	–	–	–	–	–	–	–	10
25	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	—	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	1
26	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	—	–	–	1	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	2
27	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	—	–	1	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	2
28	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	—	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
Totals	2	2	2	0	6	12	16	18	26	32	44	56	66	94	102	118	170	150	142	138	134	136	136	68	94	72	46	24	18	18	28	8	8	8	4	0	0	2	2000

TABLE IV., 1906. *Length of Fruit and Number of Ovules on First and on Second Placenta.*

Ovules on First and on Second Placenta.

Length of Fruit.	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	Totals
21—23	—	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
24—26	—	1	—	1	—	—	4	—	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8
27—29	—	2	4	10	10	10	6	4	3	3	—	—	1	1	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	56
30—32	—	2	7	19	32	27	24	21	6	7	7	1	3	2	1	1	—	—	1	—	—	—	1	—	—	—	—	—	—	—	—	162
33—35	—	2	11	28	41	56	41	35	22	28	12	9	9	16	9	4	2	4	2	3	1	1	-	—	—	—	—	—	—	—	—	336
36—38	1	—	3	13	40	51	49	45	35	25	19	14	21	17	16	6	5	1	—	1	2	—	—	—	1	1	—	—	—	—	—	366
39—41	—	1	3	10	16	26	47	46	41	28	26	24	10	10	8	4	7	5	2	1	—	1	—	—	2	—	—	—	—	—	—	318
42—44	—	—	—	2	8	19	34	38	33	35	30	20	27	12	7	17	9	4	4	2	2	—	2	1	—	—	—	—	2	—	—	308
45—47	—		2	2	4	7	12	19	16	18	25	21	10	14	12	3	11	11	3	2	5	3	3	—	1	—	2	1	1	—	—	208
48—50	—	—	—	—	3	4	5	6	4	16	16	19	13	6	12	9	8	5	4	5	4	—	3	—	—	—	—	—	—	—	—	142
51—53	—	—	—	—	—	2	4	2	1	3	1	6	2	7	4	1	4	3	2	1	1	4	1	—	1	—	—	—	—	—	—	50
54—56	—	—	—	—	—	—	—	—	—	—	—	1	3	2	2	1	2	—	3	3	1	1	1	—	—	—	—	—	—	—	—	20
57—59	—	—	—	—	—	—	—	—	—	—	1	—	1	3	—	3	—	—	1	1	1	—	1	—	—	1	1	1	1	1	1	18
60—62	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	—	—	—	—	—	—	1	—	1	4
63—65	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	2
Totals	1	8	30	86	155	202	226	216	162	163	138	115	100	90	72	49	49	33	23	20	17	10	12	1	5	2	5	2	5	1	2	2000

TABLE IV., 1907. *Length of Fruit and Number of Ovules on First and on Second Placenta.*

Ovules on First and on Second Placenta.

Length of Fruit.	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	Totals
24—26	—	1	1	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
27—29	—	—	—	1	—	—	—	—	—	1	—	—	—	—		—	—	—	—	—	—	—	—	—	—	2
30—32	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
33—35	—	—	2	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
36—38	2	3	3	1	—	2	1	—	—	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	14
39—41	1	1	6	7	5	3	3	2	1	2	—	—	2	1	—	-	—	—	—	—	—	—	—	—	—	34
42—44	—	1	5	16	16	13	8	5	2	2	2	2	2	—	—	-	—	—	—	—	—	—	—	—	—	74
45—47	—	1	6	11	11	21	19	13	13	8	2	5	—	—	—	1	—	—	1	—	—	—	—	—	—	112
48—50	—	—	2	9	18	20	24	20	11	6	5	2	1	—	—	—	—	—	—	—	—	—	—	—	—	118
51—53	—	—	1	—	7	18	26	11	8	12	16	3	2	2	2	—	—	—	—	—	—	—	—	—	—	108
54—56	—	—	—	—	6	8	17	17	13	15	14	14	9	7	4	4	1	1	1	—	1	—	—	—	—	132
57—59	—	—	—	—	1	4	10	10	14	6	4	6	4	5	1	2	5	—	—	—	1	1	—	—	—	74
60—62	—	—	—	1	2	1	5	5	8	8	3	4	—	2	2	5	—	2	1	1	—	—	—	—	—	50
63—65	—	—	—	—	—	2	2	2	2	3	2	4	3	1	—	1	2	1	1	—	—	1	1	—	2	30
66—68	—	—	—	—	—	—	—	1	1	1	2	1	5	2	3	2	3	1	1	1	—	—	—	2	2	26
69—71	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	—	—	2	—	—	—	—	—	4
72—74	—	—	—	—	—	—	—	—	—	—	—	1	—	1	2	2	1	—	—	1	—	—	—	—	—	8
75—77	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	—	—	—	1	1	—	4
78—80	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	2
Totals	3	7	26	47	67	94	115	86	73	64	50	43	29	22	15	18	13	5	5	5	2	4	2	1	4	800

TABLE V., 1906.

Length of Fruit and Number of Seeds Developing on First and on Second Placenta.

Seeds Developing on First and on Second Placenta.

Length of Fruit.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	Totals
21—23	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
24—26	—	—	—	—	2	2	2	—	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8
27—29	1	2	1	2	2	6	6	10	8	8	6	2	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	56
30—32	—	—	—	2	4	6	21	22	26	32	22	12	6	4	1	2	—	—	1	1	—	—	—	—	—	—	—	—	—	162
33—35	—	2	1	1	4	5	18	34	41	55	44	42	29	27	11	6	4	2	5	3	—	2	—	—	—	—	—	—	—	336
36—38	—	—	1	—	4	9	10	22	36	51	62	48	36	26	15	14	11	11	5	2	1	—	1	—	1	—	—	—	—	366
39—41	—	—	—	2	—	1	10	10	18	46	53	37	41	34	24	14	5	7	7	3	4	—	1	1	—	—	—	—	—	318
42—44	—	—	2	—	—	1	5	9	9	28	33	38	42	30	31	26	18	12	10	5	2	2	3	—	1	—	1	—	—	308
45—47	—	—	—	—	—	—	3	5	8	10	10	24	26	23	21	17	13	14	13	8	4	3	2	1	2	—	1	—	—	208
48—50	—	—	—	1	—	1	1	2	3	4	15	9	10	10	17	17	12	10	5	12	6	3	4	—	—	—	—	—	—	142
51—53	—	—	—	—	—	—	—	1	1	—	1	7	3	4	4	5	4	3	4	2	4	1	3	1	—	1	—	—	1	50
54—56	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	3	3	2	2	2	1	—	1	—	3	—	—	—	—	20
57—59	—	—	—	—	—	—	—	—	—	—	—	1	—	3	1	1	1	1	3	2	1	—	—	1	2	—	—	1	—	18
60—62	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	1	—	—	1	—	4
63—65	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	—	2
Totals	1	4	6	9	16	31	76	115	150	236	246	221	196	162	125	105	71	62	55	42	23	12	15	5	10	1	2	2	1	2000

TABLE V., 1907.

Length of Fruit and Number of Seeds Developing on First and on Second Placenta.

Seeds Developing on First and on Second Placenta.

Length of Fruit.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	Totals
24—26	2	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
27—29	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
30—32	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
33—35	—	1	1	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
36—38	1	—	—	6	2	3	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	14
39—41	—	—	—	4	6	6	4	6	2	2	—	1	1	—	—	1	1	—	—	—	—	—	—	—	—	—	—	34
42—44	—	2	5	6	8	12	9	14	4	3	6	1	1	1	1	1	—	—	—	—	—	—	—	—	—	—	—	74
45—47	—	—	—	2	2	8	13	13	19	16	15	13	1	4	1	2	2	1	—	—	—	—	—	—	—	—	—	112
48—50	—	—	—	—	—	5	12	20	20	21	22	7	9	—	1	—	1	—	—	—	—	—	—	—	—	—	—	118
51—53	—	—	1	2	2	2	7	11	14	16	13	16	10	5	7	1	1	—	—	—	—	—	—	—	—	—	—	108
54—56	—	1	—	—	2	1	—	7	8	14	18	17	22	12	11	13	1	2	1	2	—	—	—	—	—	—	—	132
57—59	—	—	—	—	—	—	2	5	7	10	9	10	12	4	3	3	3	1	1	1	1	—	1	—	—	1	—	74
60—62	—	—	—	—	—	1	—	2	4	4	6	8	9	5	—	1	1	2	5	2	—	—	—	—	—	—	—	50
63—65	—	—	—	—	—	—	1	—	5	2	4	3	—	6	2	—	1	—	—	1	1	—	1	—	1	1	1	30
66—68	—	—	—	—	—	—	—	—	—	—	1	1	3	1	6	3	2	4	3	1	1	—	—	—	—	—	—	26
69—71	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	1	—	—	1	—	—	—	4
72—74	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	2	—	3	1	—	—	—	—	—	—	—	8
75—77	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	1	—	—	1	—	—	—	—	—	—	4
78—80	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	2
Totals	4	5	7	23	23	38	50	78	83	88	94	77	69	40	32	26	16	12	13	8	5	0	2	2	2	2	1	800

TABLE VI., 1906.

Ovules Formed and Number of Seeds Developing per Placenta.

Seeds Developing per Placenta.

Ovules per Placenta.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	Totals
6	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
7	—	—	—	—	—	—	3	5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8
8	—	—	—	1	3	2	4	11	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	30
9	—	—	2	1	1	6	14	19	25	18	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	86
10	—	1	1	1	3	5	16	24	33	49	22	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	155
11	—	—	—	1	2	3	15	23	26	50	45	37	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	202
12	—	1	—	1	2	6	6	9	28	42	52	52	27	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	226
13	1	—	—	2	1	4	8	9	17	35	40	41	41	17	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	216
14	—	—	3	1	2	—	2	8	4	16	25	26	35	27	13	—	—	—	—	—	—	—	—	—	—	—	—	—	—	162
15	—	1	—	—	—	—	2	1	2	4	27	33	32	32	19	10	—	—	—	—	—	—	—	—	—	—	—	—	—	163
16	—	—	—	—	—	2	1	2	2	4	12	12	19	32	25	21	6	—	—	—	—	—	—	—	—	—	—	—	—	138
17	—	—	—	—	—	—	1	—	1	6	4	2	12	15	27	20	15	12	—	—	—	—	—	—	—	—	—	—	—	115
18	—	—	—	—	1	1	1	—	1	5	8	8	10	10	11	17	13	10	4	—	—	—	—	—	—	—	—	—	—	100
19	—	—	—	1	—	1	1	3	1	4	4	3	9	8	12	13	10	8	10	2	—	—	—	—	—	—	—	—	—	90
20	—	—	—	—	—	—	1	1	—	1	6	4	6	11	8	9	5	10	6	3	1	—	—	—	—	—	—	—	—	72
21	—	—	—	—	—	—	—	—	1	—	—	1	1	1	5	8	9	10	7	4	1	1	—	—	—	—	—	—	—	49
22	—	—	—	—	—	1	—	—	—	2	1	2	3	3	3	2	6	5	6	10	3	1	1	—	—	—	—	—	—	49
23	—	1	—	—	—	—	—	—	—	—	—	—	1	1	1	2	5	3	7	6	6	—	—	—	—	—	—	—	—	33
24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	1	4	4	3	4	4	—	1	—	—	—	—	23
25	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	—	4	3	3	2	3	2	—	—	—	—	—	20
26	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	3	5	1	2	2	—	2	—	—	—	—	17
27	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	2	1	1	1	—	3	—	1	—	—	—	10
28	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	—	1	2	1	2	—	3	—	1	—	—	12
29	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1
30	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	2	—	—	—	1	5
31	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	—	—	—	—	—	—	2
32	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	2	—	—	1	—	—	—	—	1	—	5
33	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	—	—	—	—	—	—	—	2
34	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	1	—	1	—	1	1	—	5
35	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
36	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	1	—	—	—	—	2
Totals	1	4	6	9	16	31	76	115	150	236	246	221	196	162	125	105	71	62	55	42	23	12	15	5	10	1	2	2	1	2000

TABLE VI., 1907.

Ovules Formed and Number of Seeds Developing per Placenta.

Seeds Developing per Placenta.

Ovules per Placenta.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	Totals
5	—	—	—	1	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
6	—	—	—	3	4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	7
7	1	—	—	7	3	6	7	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	26
8	2	—	1	3	3	9	8	12	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	47
9	—	2	1	2	2	9	9	17	17	8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	67
10	1	1	1	3	3	5	8	15	16	27	14	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	94
11	—	—	3	1	2	3	7	11	15	25	32	16	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	115
12	—	—	—	1	3	2	3	9	11	8	21	18	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	86
13	—	—	—	1	—	1	5	7	3	6	12	16	17	5	—	—	—	—	—	—	—	—	—	—	—	—	—	73
14	—	1	—	—	1	1	1	2	2	7	10	14	11	11	3	—	—	—	—	—	—	—	—	—	—	—	—	64
15	—	—	1	—	—	—	—	1	2	4	3	4	10	9	14	2	—	—	—	—	—	—	—	—	—	—	—	50
16	—	1	—	—	—	1	—	2	3	3	1	4	8	7	5	7	1	—	—	—	—	—	—	—	—	—	—	43
17	—	—	—	1	—	—	2	—	2	—	1	1	7	4	1	6	2	2	—	—	—	—	—	—	—	—	—	29
18	—	—	—	—	—	—	—	—	1	—	—	—	5	1	1	4	7	1	2	—	—	—	—	—	—	—	—	22
19	—	—	—	—	—	—	—	—	—	—	—	1	—	1	2	—	2	4	3	2	—	—	—	—	—	—	—	15
20	—	—	—	—	1	—	—	—	1	—	—	2	—	—	1	3	3	1	6	—	—	—	—	—	—	—	—	18
21	—	—	—	—	—	—	—	—	1	—	—	1	1	—	1	1	1	1	—	3	3	—	—	—	—	—	—	13
22	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	1	—	1	1	—	—	—	—	—	—	5
23	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	2	1	—	—	—	—	—	—	—	—	5
24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	2	1	—	—	1	—	—	—	5
25	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1	—	—	—	—	2
26	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	1	—	4
27	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	1	2
28	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	1
29	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	—	1	1	—	4
Totals	4	5	7	23	23	38	50	78	83	88	94	77	69	40	32	26	16	12	13	8	5	0	2	2	2	2	1	800

TABLE VII., 1906. *Total Number of Ovules Formed and* Total Seeds Developing

Total Ovules per Fruit.

	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*
15	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	—	—	—	—	—	—	—	—	—	—
16	—	—	—	—	—	—	—	—	—	—	1	—	4	—	2	1	—	—	—	—	—	—	—	—	—	—
17	—	—	—	—	—	—	—	1	—	—	1	1	1	1	4	3	2	—	—	—	—	—	—	—	—	—
18	—	—	—	—	—	—	—	1	1	—	—	1	1	2	1	2	5	1	—	—	—	—	—	—	—	—
19	—	—	—	—	1	—	—	1	—	—	2	2	2	3	7	4	5	1	1	—	—	—	—	—	—	—
20	—	—	—	—	—	1	—	1	—	1	—	2	3	3	9	2	5	4	2	2	—	—	—	—	—	—
21	—	—	—	—	—	—	1	—	—	1	—	1	—	—	5	4	5	9	10	8	4	—	—	—	—	—
22	—	—	—	—	—	—	—	—	—	1	—	—	1	2	9	3	10	3	12	5	6	6	—	—	—	—
23	—	—	—	—	—	—	—	—	—	1	1	1	2	1	1	6	8	6	7	7	11	2	—	—	—	—
24	—	—	—	—	—	—	—	—	—	2	—	2	1	2	3	5	5	4	4	9	6	11	3	1	—	—
25	—	—	—	—	—	—	—	1	—	2	—	—	1	3	—	3	3	4	5	8	5	8	9	—	3	—
26	—	—	1	—	—	—	—	—	—	—	—	—	1	1	1	—	4	1	3	6	10	10	6	5	5	—
27	—	—	—	—	—	—	—	—	—	—	1	—	—	1	2	3	2	3	4	6	6	4	12	4	2	3
28	1	—	—	1	—	—	—	—	—	—	—	—	—	—	1	—	—	2	6	5	6	2	7	1	4	2
29	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	2	—	4	2	6	2	5	4	6	5
30	—	—	—	—	—	—	—	—	2	—	—	—	1	—	—	—	—	1	1	1	—	1	10	7	8	2
31	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	2	2	—	2	2	4	5	6	2
32	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1	—	3	3	1	2	3	2	5
33	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1	—	—	—	1	—	5	2	5
34	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	2	1	—	4	2
35	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	1	—	1	—	1	2
36	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	1	1	1	—	2	4
37	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	2	—	—	1	—	—	—	3	2
38	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1	—	1	1	1	—	2	—
39	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1	—	—	2	2	—	—	—
40	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1	1	2	2
41	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	1	—	—	—	—	1	—	—	—
42	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—
43	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	1
44	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
45	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
46	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—
47	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
48	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
49	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
50	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
51	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
52	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
53	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
54	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
55	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
56	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
57	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
58	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
59	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
60	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
61	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
62	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
63	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
64	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
65	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
66	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
67	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
68	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
69	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
70	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Totals	1	0	1	1	2	1	1	5	3	9	6	12	22	21	47	38	58	49	64	63	70	56	68	38	53	39

Total Number of Seeds Developing per Fruit.

per Fruit.

27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	Totals
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	14
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	15
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	29
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	35
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	48
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	58
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	54
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	58
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	55
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	54
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	53
2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	40
3	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	42
1	4	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	40
5	4	3	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	42
4	1	3	3	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	35
2	5	4	3	1	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	32
6	2	3	4	3	3	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	32
3	2	1	3	5	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	24
—	4	4	1	2	1	2	1	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	28
1	1	1	3	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	17
3	—	—	—	1	1	2	2	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	17
—	2	4	3	2	1	1	1	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	22
1	1	3	—	1	3	3	—	1	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	21
2	—	—	—	—	1	—	—	2	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10
1	1	—	3	1	2	1	2	3	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	19
—	1	—	—	1	—	2	1	3	1	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	15
—	1	—	—	—	2	—	—	1	2	1	1	1	—	—	1	—	—	—	—	—	—	—	—	—	10
—	—	—	—	1	—	—	—	—	2	—	—	2	—	—	1	—	—	—	—	—	—	—	—	—	7
—	—	—	—	1	—	1	2	—	—	1	1	1	—	—	—	—	—	—	—	—	—	—	—	—	8
—	—	—	—	—	—	—	—	—	2	2	1	—	1	—	—	—	—	—	—	—	—	—	—	—	6
—	—	—	—	—	—	—	1	1	—	—	1	1	—	1	—	—	—	—	—	—	—	—	—	—	5
—	—	—	—	—	—	—	—	1	—	1	1	1	2	—	—	—	1	—	—	—	—	—	—	—	7
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1	—	—	—	3
—	—	—	—	1	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	2	—	—	—	—	4
—	—	—	—	—	—	—	1	—	1	1	—	—	1	1	—	2	—	—	—	—	—	—	—	—	7
—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	2
—	—	—	—	—	—	—	—	—	1	—	—	2	—	2	—	—	—	1	—	—	—	—	—	—	7
—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	—	—	—	—	—	1	—	—	3
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	2
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	2
34	30	27	25	24	19	14	15	15	12	12	7	9	6	4	3	4	2	1	1	3	1	1	2	1	1000

TABLE VII., 1907. *Total Number of Ovules Formed and*

Total Seeds Developing

Total Ovules per Fruit.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
11							1																		
12								1			1														
13							1				1														
14							1				1														
15	1					1			1	1	1	1	2	1		1									
16								2	3	1		3	1	1	2	1									
17							1				2	1	1	1	4	1	1	1							
18							1	1	1		1	1	3	2	3	1	2	3	1						
19				1							1	1	2		3	4	3	5							
20				2							1	3			1	1	1	3	3	2					
21													2	1	1	1	1	6	1	10	5				
22		1				1	1						1		2	1	2	3	3	4	3	3	3		
23									1	1			1	1		1	2	2	4	2	4	2	2	1	
24											1			3	1	4	1	4	3	4	5	5	6	3	
25										1		1					2	3		1	2	1	6	2	1
26														1			1	1		1	4	3	2	3	1
27									1		1				1		2					4	1	1	3
28																	1	1					1	1	2
29														1	1		1		2					3	1
30												1			1						1				2
31																							1		
32																					2	1	1		
33									1							1		2		1				1	
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58																									
Totals	1	1	0	3	0	2	6	4	8	4	11	12	14	13	21	17	20	34	17	26	26	19	22	17	10

Total Number of Seeds Developing per Fruit.

per Fruit.

25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	Totals
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	14
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	13
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	20
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	20
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	17
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	28
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	28
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	24
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	40
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	20
2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	19
—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	15
2	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	9
7	1	4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	21
3	—	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10
—	4	—	1	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8
—	—	2	1	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8
1	1	—	1	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	11
1	—	4	—	—	2	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10
—	2	—	2	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	5
—	—	—	1	—	1	—	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	5
—	—	—	—	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
—	1	—	—	—	1	—	—	—	1	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	5
1	—	—	1	—	—	—	—	1	1	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	5
1	—	—	—	—	—	—	—	—	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	3
—	1	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	3
—	1	—	—	—	—	—	—	—	—	1	—	—	—	2	—	—	—	—	—	—	—	—	—	—	4
—	—	—	—	—	1	—	—	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	3
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	1	—	—	—	—	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	3
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	1	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	2
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	2
18	12	11	12	3	7	2	2	3	4	3	5	3	0	2	0	0	0	1	0	0	0	2	1	1	400

TABLE VIII., 1906.

Number of Ovules on First Placenta and Number of Ovules on Second Placenta.

Ovules on Second Placenta.

Ovules on First Placenta.	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	Totals
6	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
7	—	—	1	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
8	—	1	3	8	2	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	16
9	—	3	5	10	13	2	3	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	37
10	—	—	3	12	27	15	7	5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	69
11	—	—	2	6	25	37	23	9	6	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	110
12	—	—	—	5	9	18	37	23	7	6	4	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	110
13	—	—	—	5	7	10	21	30	19	9	5	3	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	110
14	—	—	—	—	2	5	14	18	15	9	6	5	2	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	78
15	—	—	—	—	—	2	8	10	21	21	15	5	3	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	87
16	—	—	—	—	—	1	2	7	7	15	16	12	7	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	69
17	—	—	—	—	—	—	—	1	4	8	13	15	7	13	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	63
18	—	—	—	—	—	—	1	2	4	3	6	9	11	8	4	2	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	52
19	—	—	—	—	—	—	—	—	—	1	3	3	8	9	6	4	4	1	—	1	—	—	—	—	—	—	—	—	—	—	—	40
20	—	—	—	—	—	—	—	1	—	3	—	—	4	10	9	3	4	1	2	1	—	—	—	—	—	—	—	—	—	—	—	38
21	—	—	—	—	—	—	—	—	—	—	—	—	3	5	1	8	5	1	—	1	—	—	—	—	—	—	—	—	—	—	—	24
22	—	—	—	—	—	—	—	—	—	—	—	—	1	2	5	5	4	2	1	—	1	—	—	—	—	—	—	—	—	—	—	21
23	—	—	—	—	—	—	—	—	—	—	—	—	—	1	4	2	3	3	1	2	2	1	—	—	—	—	—	—	—	—	—	19
24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	3	3	1	3	—	1	2	—	—	—	—	—	—	—	—	14
25	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	1	2	—	2	1	1	—	1	—	—	—	—	—	—	10
26	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	2	—	2	—	—	—	—	—	—	—	—	5
27	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	2	1	—	—	—	—	—	—	—	—	5
28	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	3	—	—	—	—	—	—	—	—	—	—	6
29	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	1
30	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1	1	—	—	—	—	—	3
31	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1
32	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	—	2
33	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
34	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	2	4
35	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1
Totals	0	4	14	49	86	92	116	106	84	76	69	52	48	50	34	25	28	14	9	10	12	5	6	0	2	1	3	2	1	0	2	1000

TABLE VIII., 1907.

Number of Ovules on First Placenta and Number of Ovules on Second Placenta.

Ovules on Second Placenta.

Ovules on First Placenta.	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	Totals
6	1	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
7	2	1	1	6	2	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	15
8	—	—	3	6	8	4	1	—	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	24
9	—	1	4	3	8	9	3	5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	33
10	—	2	—	5	8	11	15	7	2	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	53
11	—	—	2	2	3	5	16	8	11	5	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	55
12	—	—	—	—	3	2	11	13	7	3	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	40
13	—	—	—	—	1	3	9	6	6	6	4	2	—	—	—	—	—	—	—	—	—	—	—	—	—	37
14	—	—	—	—	—	3	1	5	5	2	12	1	1	—	—	—	—	—	—	—	—	—	—	—	—	30
15	—	—	—	—	1	1	2	2	2	4	4	2	1	2	—	—	—	—	—	—	—	—	—	—	—	21
16	—	—	—	—	—	1	1	—	3	5	2	4	3	3	—	—	—	—	—	—	—	—	—	—	—	22
17	—	—	—	—	—	—	—	—	—	3	2	4	2	2	1	2	—	—	—	—	—	—	—	—	—	16
18	—	—	—	—	—	—	—	—	—	1	2	4	2	1	—	2	—	—	—	—	—	—	—	—	—	12
19	—	—	—	—	—	—	—	—	—	—	1	1	2	1	1	2	1	—	—	1	—	—	—	—	—	10
20	—	—	—	—	—	—	—	—	—	—	—	1	—	1	3	1	3	1	—	—	—	—	—	—	—	10
21	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	3	1	—	—	—	—	—	—	—	5
22	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	1	—	—	—	—	3
23	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	—	—	—	—	—	—	—	—	2
24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	1	—	—	—	—	—	3
25	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1
26	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	—	2
27	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
28	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1
29	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	2
Totals	3	4	11	23	34	41	60	46	36	34	29	21	13	10	5	8	8	2	3	2	1	2	2	0	2	400

TABLE IX., 1906.

Number of Seeds Developing on First Placenta and Number of Seeds Developing on Second Placenta.

Seeds Developing on Second Placenta.

Seeds Developing on First Placenta.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	Totals
1	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
2	—	1	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3.
3	—	—	—	—	—	1	—	—	1	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4
4	—	—	—	1	2	3	1	1	1	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10
5	—	1	—	1	—	4	2	1	3	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	13
6	—	—	1	—	2	2	7	6	5	4	2	3	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	33
7	—	—	—	1	—	1	11	8	12	9	5	4	—	—	1	—	1	—	—	—	—	—	—	—	—	—	—	—	53
8	—	—	1	—	1	—	4	16	12	23	8	6	3	4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	78
9	—	—	—	1	—	3	13	10	14	22	23	13	5	2	3	1	—	—	—	—	—	—	—	—	—	—	—	—	110
10	—	—	—	—	—	2	3	12	10	24	20	20	11	12	7	2	2	1	—	—	—	—	—	—	—	—	—	—	126
11	—	—	—	—	—	1	1	3	7	22	24	22	18	8	4	4	1	—	—	1	—	—	—	—	—	—	—	—	116
12	—	—	—	—	—	—	1	4	4	12	14	20	13	23	11	7	—	2	—	—	—	—	—	—	—	—	—	—	111
13	—	—	—	—	—	—	—	1	3	7	12	7	16	8	5	4	5	3	4	1	—	—	—	—	—	—	—	—	76
14	—	—	—	—	—	—	—	—	—	2	2	2	10	12	15	5	5	3	1	—	2	—	—	—	—	—	—	—	59
15	—	—	—	—	—	—	—	—	—	—	5	4	4	9	11	10	3	4	2	2	—	—	1	—	—	—	—	—	55
16	—	—	—	—	—	1	—	—	—	1	—	2	2	1	3	7	7	6	2	4	1	1	—	—	—	—	—	—	38
17	—	—	—	—	—	—	—	—	—	—	2	—	2	3	4	3	4	6	5	1	—	—	1	—	—	—	—	—	31
18	—	—	—	—	—	—	—	—	—	—	—	1	—	3	2	2	2	3	5	3	3	—	2	—	1	—	—	—	27
19	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	2	2	5	3	2	—	1	—	1	—	—	—	18
20	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	1	5	—	—	1	1	—	—	—	1	12
21	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	1	2	2	1	—	—	1	—	—	—	9
22	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	2	1	—	—	1	—	—	1	—	—	6
23	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	1	—	1	—	4
24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	1	—	—	—	3
25	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
26	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1
27	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1
28	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	1
Totals	1	2	3	5	6	18	43	62	72	126	120	105	85	86	66	50	33	31	28	24	11	3	9	1	7	1	1	1	1000

TABLE IX., 1907.

Number of Seeds Developing on First Placenta and Number of Seeds Developing on Second Placenta.

Seeds Developing on Second Placenta.

Seeds Developing on First Placenta.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	Totals
0	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2
1	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
2	—	2	—	—	1	1	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	5
3	1	—	1	5	3	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	13
4	—	1	—	—	2	2	3	4	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	13
5	—	—	—	2	2	4	3	7	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	20
6	—	—	—	—	1	3	3	5	7	1	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	22
7	—	—	—	3	1	3	7	7	8	8	4	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	42
8	—	—	—	—	—	—	4	4	6	11	6	1	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	35
9	—	—	1	—	—	1	3	2	13	3	10	8	3	2	1	—	—	—	—	—	—	—	—	—	—	—	—	47
10	—	—	—	—	—	1	2	5	7	9	10	6	8	2	—	—	—	—	—	—	—	—	—	—	—	—	—	50
11	—	—	—	—	—	—	1	1	5	5	5	8	8	4	5	—	—	—	—	1	—	—	—	—	—	—	—	43
12	—	—	—	—	—	—	—	—	1	2	4	5	4	6	1	3	1	—	—	—	—	—	—	—	—	—	—	27
13	—	—	—	—	—	—	—	—	—	—	1	1	6	4	1	1	—	—	—	—	—	—	—	—	—	—	—	14
14	—	—	—	—	—	—	—	1	—	—	1	1	4	2	5	1	1	1	—	—	—	—	—	—	—	—	—	17
15	—	—	—	—	—	—	1	—	—	—	—	3	3	3	1	3	—	—	—	—	—	—	—	—	—	—	—	14
16	—	—	—	—	—	—	—	—	—	—	—	1	2	1	1	1	1	—	3	—	—	—	—	—	—	—	—	10
17	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	1	2	—	1	1	2	—	—	—	—	—	—	9
18	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	3	1	—	—	—	—	—	—	—	6
19	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	—	—	—	—	—	—	—	—	3
20	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	1	—	—	—	3
21	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
22	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	2
23	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1
24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1
Totals	2	4	2	10	10	18	28	36	48	41	44	34	42	26	15	12	6	3	7	5	2	0	0	1	1	2	1	400

TABLE X., 1906.

Number of Aborted Ovules on First Placenta and Number of Aborted Ovules on Second Placenta.

Aborted Ovules on Second Placenta.

Aborted Ovules on First Placenta.	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	Totals
0	27	21	23	11	6	1	1	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	91
1	31	55	32	28	13	8	4	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	173
2	14	37	39	43	24	10	6	4	2	—	1	—	—	1	—	—	—	—	—	—	—	—	—	—	181
3	17	22	37	28	27	12	9	6	2	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	161
4	5	12	19	33	27	18	6	10	6	2	2	—	—	—	—	—	—	—	—	—	—	—	—	—	140
5	1	7	19	13	20	13	9	7	3	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	93
6	—	5	2	6	4	8	9	3	2	1	2	—	1	—	—	—	—	—	—	—	—	—	—	—	43
7	—	1	2	5	2	5	4	8	5	2	2	1	2	—	—	—	—	—	—	—	—	—	—	—	39
8	1	—	—	1	4	3	3	5	—	—	2	1	—	—	—	—	—	—	—	—	—	—	—	—	20
9	—	—	2	2	—	2	1	5	4	2	1	1	1	—	—	—	—	—	—	—	—	—	—	—	21
10	—	—	—	—	—	1	2	2	1	1	1	2	—	1	—	—	1	—	—	—	—	—	—	—	12
11	—	—	—	—	—	2	1	—	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6
12	—	—	—	1	—	—	—	—	1	—	1	1	2	—	—	—	—	—	—	—	—	—	—	—	6
13	—	—	—	—	—	—	2	—	—	—	—	—	1	2	1	1	—	—	—	—	—	—	—	—	7
14	—	—	—	—	—	1	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	—	—	3
15	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
16	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
17	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	1
18	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
19	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
20	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
21	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	1
22	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1	2
Totals	96	160	175	171	127	84	57	52	29	10	13	6	7	6	2	1	2	0	1	0	0	0	0	1	1000

TABLE X., 1907.

Number of Aborted Ovules on First Placenta and Number of Aborted Ovules on Second Placenta.

Aborted Ovules on Second Placenta.

Aborted Ovules on First Placenta.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Totals
0	9	14	10	3	—	1	—	—	—	—	—	—	—	—	—	—	37
1	12	29	25	9	8	3	1	—	—	—	—	—	—	—	—	—	87
2	12	16	24	12	4	6	—	1	—	—	—	—	—	—	—	—	75
3	4	16	17	12	4	3	4	—	1	—	—	—	—	—	—	—	61
4	2	8	11	10	10	4	3	2	1	—	—	—	—	—	—	—	51
5	1	3	2	5	5	6	5	1	—	1	—	—	—	—	—	—	29
6	—	2	1	4	3	4	—	1	2	1	3	—	—	1	—	—	22
7	—	—	2	—	3	2	1	1	1	—	2	1	—	—	—	—	13
8	—	—	—	—	1	2	—	1	1	3	—	—	—	1	—	—	9
9	—	—	—	—	—	1	1	4	2	—	1	—	—	—	—	—	9
10	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	1
11	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	2
12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
13	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
14	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
15	—	—	—	—	—	—	1	—	—	—	—	—	1	—	—	1	3
16	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1
Totals	40	88	92	55	38	33	16	11	8	5	6	1	2	3	1	1	400

TABLE XII., 1906 AND 1907.

Seriation of Total Number of Aborted Ovules per Placenta.

Grade	1906	1907	Grade	1906	1907
0	27	9	25	1	1
1	52	26	26	3	—
2	92	51	27	3	1
3	97	48	28	1	—
4	100	51	29	—	1
5	107	47	30	—	1
6	87	33	31	1	—
7	98	25	32	—	—
8	64	19	33	—	—
9	60	20	34	—	—
10	36	12	35	—	—
11	34	15	36	—	—
12	34	5	37	—	—
13	16	4	38	1	—
14	18	5	39	1	—
15	15	5	40	—	—
16	13	8	41	—	—
17	9	7	42	—	—
18	7	1	43	—	—
19	11	2	44	—	—
20	3	—	45	1	—
21	3	2			
22	1	—			
23	2	1	Totals	1000	400
24	2	—			

TABLE XI., 1906.

Number of Ovules on "First" Placenta and Number of Seeds Developing on "Second" Placenta.

Seeds Developing on "Second" Placenta.

Ovules on "First" Placenta.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	Totals
6	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1
7	—	—	—	—	—	1	2	1	2	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8
8	—	—	—	—	1	2	3	7	7	9	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	30
9	—	—	1	1	3	3	12	18	17	13	9	5	—	4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	86
10	—	1	1	2	4	6	15	23	32	21	23	20	7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	155
11	—	—	—	1	2	3	12	24	35	50	28	31	9	6	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	202
12	—	—	1	2	2	5	13	14	13	49	46	44	16	17	3	1	—	—	—	—	—	—	—	—	—	—	—	—	—	226
13	—	1	1	—	—	5	8	14	13	33	37	38	42	11	6	3	3	1	—	—	—	—	—	—	—	—	—	—	—	216
14	—	1	2	—	—	2	6	4	11	21	32	16	24	20	14	7	1	1	—	—	—	—	—	—	—	—	—	—	—	162
15	1	—	—	1	1	1	1	2	11	13	19	22	33	27	11	12	3	3	2	—	—	—	—	—	—	—	—	—	—	163
16	—	—	—	—	2	—	1	—	4	10	21	16	18	10	24	17	9	3	3	—	—	—	—	—	—	—	—	—	—	138
17	—	—	—	—	—	—	—	3	—	3	8	6	15	19	27	13	8	9	2	2	—	—	—	—	—	—	—	—	—	115
18	—	—	—	1	—	1	—	2	2	6	11	8	8	13	15	16	9	5	1	1	—	—	1	—	—	—	—	—	—	100
19	—	—	—	1	—	1	2	1	—	4	3	4	11	12	9	12	7	14	3	4	—	1	1	—	—	—	—	—	—	90
20	—	—	—	—	—	—	1	—	1	—	8	7	5	11	4	4	9	5	9	5	1	—	2	—	—	—	—	—	—	72
21	—	—	—	—	—	—	—	1	—	1	1	1	2	6	3	6	9	5	8	4	1	1	—	—	—	—	—	—	—	49
22	—	—	—	—	—	1	—	1	—	1	—	2	5	—	4	7	7	5	6	5	4	—	1	—	—	—	—	—	—	49
23	—	—	—	—	1	—	—	—	—	—	—	—	1	2	2	—	3	5	7	4	3	3	1	1	—	—	—	—	—	33
24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	4	—	1	4	4	2	2	1	1	2	1	—	—	—	23
25	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1	1	2	—	5	4	2	—	1	3	—	—	—	—	20
26	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	2	—	2	1	3	1	3	1	2	—	—	—	1	17
27	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3	—	3	1	1	—	—	—	1	—	—	10
28	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	2	—	—	1	4	—	1	1	1	—	—	—	—	—	12
29	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	1
30	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	1	1	—	—	1	—	—	—	—	—	—	5
31	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	1	—	2
32	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	2	—	1	—	—	—	—	—	—	5
33	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	2
34	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	1	—	2	—	1	—	—	5
35	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	1
36	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	2
Totals	1	4	6	9	16	31	76	115	150	236	246	221	196	162	125	105	71	62	55	42	23	12	15	5	10	1	2	2	1	2000

TABLE XI., 1907.

Number of Ovules on "First" Placenta and Number of Seeds Developing on "Second" Placenta.

Seeds Developing on "Second" Placenta.

Ovules on "First" Placenta.	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	Totals
5	—	—	—	2	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
6	—	—	—	4	—	—	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	7
7	1	—	1	4	4	5	2	5	3	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	26
8	1	1	—	4	4	6	5	13	2	7	3	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	47
9	—	—	3	4	4	7	6	13	12	7	6	2	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	67
10	1	1	—	1	5	11	10	9	15	11	20	4	6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	94
11	—	1	1	1	3	2	10	11	17	18	24	15	8	2	1	1	—	—	—	—	—	—	—	—	—	—	—	115
12	—	1	1	—	—	2	5	13	12	16	12	14	6	3	1	—	—	—	—	—	—	—	—	—	—	—	—	86
13	—	—	—	—	2	1	3	5	6	11	11	17	9	4	4	—	—	—	—	—	—	—	—	—	—	—	—	73
14	1	—	—	1	—	2	1	4	9	7	7	4	7	9	7	3	1	1	—	—	—	—	—	—	—	—	—	64
15	—	—	1	1	—	—	2	3	1	4	6	5	11	7	4	1	3	1	—	—	—	—	—	—	—	—	—	50
16	—	—	—	1	—	—	3	1	3	3	3	5	9	4	4	5	1	1	—	—	—	—	—	—	—	—	—	43
17	—	—	—	—	—	1	—	—	1	2	1	4	3	5	2	6	1	1	1	1	—	—	—	—	—	—	—	29
18	—	1	—	—	—	—	—	—	—	1	—	4	2	2	1	3	4	2	2	—	—	—	—	—	—	—	—	22
19	—	—	—	—	—	—	—	—	—	—	1	1	2	1	—	3	1	—	4	2	—	—	—	—	—	—	—	15
20	—	—	—	—	—	—	1	—	2	—	—	—	1	1	3	—	3	2	3	1	1	—	—	—	—	—	—	18
21	—	—	—	—	1	—	—	—	—	—	—	1	1	—	1	1	—	2	—	3	3	—	—	—	—	—	—	13
22	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	—	2	1	—	—	—	—	—	—	—	—	5
23	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	1	—	1	1	—	—	—	—	—	—	—	5
24	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1	—	1	—	1	—	—	1	—	—	—	5
25	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	1	—	2
26	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	1	4
27	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	1	—	—	—	—	2
28	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	1
29	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	—	1	1	—	4
Totals	4	5	7	23	23	38	50	78	83	88	94	77	69	40	32	26	16	12	13	8	5	0	2	2	2	2	1	800

AN OBJECTIVE STUDY OF MATHEMATICAL INTELLIGENCE*.

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I. *Introductory.*

One of the most pressing needs of the psychology of the present day is a theory of reasoning, full, concrete, and capable of practical application. If we turn to the chapters on reasoning in any of the standard text-books, we find a vagueness and diffidence strongly contrasting with the wealth of detail and confident generalisation prominent in the chapters on lower mental processes†. This state of affairs is due in part, doubtless, to the desire not to encroach on the territory of the logician, in part to the complexity of the processes concerned,—most of all, however, to the insufficiency of the method employed, viz. that of unaided introspection. That this is so, is clear from the fact that the psychology of thought has even a longer history than that of most of the simpler mental processes, and yet has made far less progress.

The unsatisfactory character of the generally-accepted introspective account has been recently brought into prominence by considerations, separately conclusive, yet quite independent of one another. These originate from the three distinct realms of normal psychology, psychopathology, and applied psychology of education, respectively.

(1) In normal psychology the evidence is derived from certain experiments, among which those of Thorndike, Woodworth, Winch, Watt, and Ach are of chief importance. They all point to the fact that mental imagery, or, in more general terms, sensory content of any kind, is not an indispensable factor in cognitive process, and, even when present, is of small efficiency. Thorndike‡ compared

* Read before the British Psychological Society, March 13, 1909. Considerable changes have since been made in the methods of evaluating the results.

† A recent notable exception is J. E. Miller's *The Psychology of Thinking*, Macmillan, 1909.

‡ E. L. Thorndike: "On the Function of Visual Images," *Journal of Philosophy, Psychology, and Scientific Methods*, Vol. iv. No. 12, p. 324, June 6, 1907.

good and bad visualisers with one another with respect to memory of facts of visual perception, and found no superiority in the case of the former class. One of Woodworth's experiments* was to present to the subject a *relation* by means of two terms, a and b, between which it held, and a third term, c, for which a fourth term, x, was to be found bearing the same relation to c that b did to a. The terms were cards of different colours and shapes, and also significant words, such as:—

London : England :: Paris : x,
Mice : cats :: worms : x.

The subjects' introspections showed that, although in difficult cases the relation may come to consciousness in the form of a name or some sort of mental imagery, in some cases the relation is felt without a name or mental image of any kind, and in easy cases the answer comes immediately, with very little consciousness at all.

Winch† tested the memory of school-children for drawings under conditions in which mental "imaging" immediately after the period of exposure was either enjoined or forbidden. No superiority in memorising-power was shown by the children under the former of the two conditions.

Watt‡ and Ach§ reached their conclusions in the course of experiments on reaction-times. It was found by both observers that the reaction was determined in the mind of the subject by the general purpose or known nature of the task set by the experimenter, often at a very low degree of consciousness. According to Ach, image and stimulus are both secondary to the purpose or task.

(2) In the realm of psychopathology there is overwhelming evidence that loss of mental functioning may take other forms than those of loss of sensation or of mental imagery. In particular, the researches of Marie‖ and his school¶ into the problem of aphasia seem to point to a priority of thought over sensory content of any kind.

(3) The practical uselessness for education of the prevalent psychological theory of reasoning is becoming more and more apparent in educational circles. Outside the walls of the infant school, education deals primarily with the training and development of reasoning as such, but this cannot be considered, after the manner of the faculty psychologists, as a general capacity functioning independently of the material on and through which it works. Each school subject has potenti-

* R. S. Woodworth: "The Consciousness of Relation," *Essays Philosophical and Psychological in Honour of William James*, 1908, pp. 489—492.

† W. H. Winch: "The Function of Images," *Journal of Philosophy, Psychology, and Scientific Methods*, Vol. v. No. 13, pp. 337 ff., June 18, 1908.

‡ H. J. Watt: *Experimentelle Beiträge zu einer Theorie des Denkens*, 1905.

§ N. Ach: *Ueber die Willenstätigkeit und das Denken*, Göttingen, 1905.

‖ Pierre Marie: *Semaine Medicale*, May 23, Oct. 17, and Nov. 28, 1906. Pierre Marie: "Sur la Fonction du Langage," *Revue de Philosophie*, 1907.

¶ François Montier: *L'Aphasie de Broca*: Paris, Steinheil, 1908. L. Dugas: "Une Théorie Nouvelle de l'Aphasie," *Journal de Psychologie*, Sept. Oct., 1908.

ally a psychology as well as a logic; but this potentiality has not as yet been actualised by the psychologists, the reason being, as is shown by the cases previously mentioned, that introspection alone is insufficient to give us the requisite data*.

Apart from the experiments mentioned earlier in this paper, most of the experimental work on the psychology of reasoning has taken the form of a study of "tests" of elementary physical and mental processes accepted problematically as symptoms of degrees of intelligence. The history of this movement is already recorded with sufficient fulness in Meumann's *Experimentelle Pädagogik*, Bd. I. (Leipzig, 1907). Hitherto the results obtained have been either conflicting or negative. In particular, the hypothesis, put forward by one investigator, that general intelligence and general power of sensory discrimination are identical, has not been confirmed by recent work.

Mention should, however, be made of the fact that the problem of reasoning has been attacked more directly in a recent research by Willis L. Gard, entitled "A Preliminary Study of the Psychology of Reasoning," communicated to the *American Journal of Psychology*† by Prof. Sanford. The experiments took the form of getting the subject to solve simple mathematical puzzles, e.g. to fill up gaps in partially worked out division and multiplication sums. It seems somewhat doubtful whether puzzles are ever a good test of intelligence, since they really involve a good deal of specialisation of interest and ability.

II. *Historical.*

Turning now to the subject-proper of this paper, viz. the study of mathematical intelligence, we find that earlier experimental work is limited to the analysis of the mental processes involved in the simplest forms of arithmetical operation,—counting, adding, subtracting, etc.

(*a*) *Counting.* As regards counting, experiments have been worked to decide the rival claims of the *intuitional method* (of Pestalozzi) and the *counting method* in the teaching of number. These two methods are based on the spatial and temporal groupings of units, respectively, and the experiments therefore fall naturally into two classes. The first includes the experiments of Cattell‡, Lay§, Warren‖, Messenger¶ and Helene Nanu**, who all worked with the tachistoscope, exposing to the subjects' view lines or dots, varying in number and arrangement, for very short lengths of time. Experiments belonging to the second class are

* See William Brown: "Educational Psychology in the Secondary School," *Journal of Philosophy, Psychology, and Scientific Methods*, Vol. VII. No. 1, Jan. 6, 1910.

† Vol. XVIII. Oct. 1907.

‡ Cattell: "Ueber die Zeit des Erkennens u.s.w." *Philos. Studien*, III. 1886.

§ Lay: *Führer durch den ersten Rechenunterricht*, 1 Aufl., 1898.

‖ Warren: *Princetown Contributions to Psychology*, II. 1898, Vol. 3.

¶ Messenger: "The Perception of Number," *Psychol. Review*, Monograph Supplement, V. 1903.

** Helene A. Nanu: "Zur Psychologie der Zahlauffassung," *Würzburger Dissertation*, 1904.

those of Dietze*, who employed a metronome and asked the subject to compare series of beats of different lengths with one another (without counting). His purpose was to determine how many beats could be correctly estimated without counting. The result was found to vary with the rapidity of the beats, the most favourable interval between the beats being ·2 to ·3 sec.

(*b*) *Simple Arithmetical Processes.* The psychology of addition, subtraction, and other simple arithmetical operations has been repeatedly studied by introspection, either casual or controlled. An example of the former is to be found in Binet's *Psychologie des Grands Calculateurs et Joueurs d'Échecs*, and of the latter in an article in Vol. XVII. of the *Am. J. P.* (1906)†.

(*c*) *Higher Mathematical Processes.* As regards the psychological analysis of higher mathematical processes, very little work has thus far been done. In almost all cases the data have been school and college marks or class-lists. The method employed has been generally that of correlation, but the coefficients of correlation have been worked out between mathematical ability as a whole and the equally complex mental capacities involved in the other main subjects of the school or college curriculum, e.g. Classics, French, English, etc. No instance of analysis of the *inner* relations of mathematical intelligence is known to the writer.

III. *Collection and Analysis of Data.*

Towards the close of the year 1908, the writer examined in mathematics a group of 83 boys belonging to the middle forms of the classical side of an English public school. The group consisted of five sets or forms, viz. U. V^{A}, U. V^{B}, L. VI, U. VI and VII; but all the boys were examined on the same three papers—Geometry, Arithmetic, and Algebra—and they had all been working along the same lines and in the same environment. The examination papers were set by one of the masters, and the writer was expected to return the boys' answers, marked, within the week. Although this arrangement meant that the work had to be done at high pressure, it was on the whole a fortunate one so far as the research was concerned, since it almost certainly rendered the standard of marking much more steady than the latter would have been if the marking had extended over a longer time.

The papers were first marked according to ordinary school standards for the sake of the school examination, and then *according to a differential system of*

* Dietze: "Ueber den Bewusstseinsumfang," *Philos. Studien*, II. 1885.

† C. E. Browne: "The Psychology of the Simple Arithmetical Processes," *Am. J. P.* Vol. XVII. Jan. 1906; see also Frank D. Mitchell: "Mathematical Prodigies," *Am. J. P.* Vol. XVIII. Jan. 1907.

Recently I have myself had excellent opportunities of studying these arithmetical processes introspectively. The processes seem to go on almost mechanically, but are yet controlled from moment to moment by auditory and kinaesthetic imagery. When any distraction occurs, either from without or from within, visual imagery is immediately called up, and this is sufficiently stable to allow me to pause for a moment and "collect my thoughts." The various forms of imagery present seem to act as controls of the thought, but otherwise to be quite inactive, the "driving power" coming from the *standing purpose* to add, subtract, etc. (Cf. supra, p. 353, Ach's results.)

marking based on an analysis of the intellectual processes involved in the answering of the questions as far as they could be detected by introspection; according to this system, the maximum mark for any one process in any one answer was 10. (For samples of questions, analyses, and actual marks, see Appendices I. and III.)

It was decided that the number of boys examined was not large enough to sustain a mathematical superstructure built on so detailed an analysis. The analyses were therefore subsumed under nine principal headings, viz., A, B, C, D, E, F, G, H, I, distributed as follows:—

Geometry.

A. Memory of definitions and general principles (e.g. principle of superposition) in Geometry.

[1 (iii) + 3 (iii) + 5 (i) + 6 (i) + 8 (i) + 9 (i)]*.

B. Memory of constructions in Geometry.

[2 (i) + 3 (ii) + 4 (i) + 5 (iii) + 9 (ii)].

C. Memory of preceding propositions, and power of applying them. Geometry.

[1 (ii) + 2 (iii) + 3 (i) + 4 (iii + v) + 5 (ii + iv + vi) + 6 (ii) + 7 (i + iv) + 8 (ii + iii)].

D. Recognition of necessity of generality in proof, and power of recognizing general relations in a particular case. Geometry.

[1 (i + iv) + 3 (iv) + 4 (iv + vi) + 5 (v) + 6 (iii) + 8 (iv)].

Arithmetic.

E. Accuracy in Arithmetic.

[1 (ii) + 2 (iii) + 3 (iv) + 4 (iii) + 5 (iv) + 6 (ii) + 7 (ii) + 8 (iii) + 9 (ii) + 10 (iv) + 11 (ii) + 12 (iii) + 13 (ii)].

F. General memory of rules and power of applying them. Arithmetic.

[1 (i) + 2 (i + ii) + 3 (ii) + 4 (i + ii) + 6 (i) + 8 (i + ii) + 10 (i + iii) + 11 (i) + 12 (i + ii) + 13 (i)].

G. Power of doing sums in percentage and proportion. Arithmetic.

[7 (i) + 9 (i) + 12 (i + ii)].

Algebra.

H. Accuracy in Algebra.

[1 (iii) + 2 (iii) + 3 (ii) + 4 (iv) + 5 (ii) + 6 (iii) + 7 (iv) + 8 (iii) + 9 (ii) + 10 (iii)].

I. General memory of rules and power of applying them. Algebra.

[1 (i + ii) + 2 (ii) + 3 (i) + 4 (ii + iii) + 5 (i) + 6 (ii) + 7 (i + ii + iii) + 8 (i)].

* See Appendix I for meaning of these numbers.

These marks, together with the ages of the boys and their total marks for Geometry, Arithmetic, and Algebra, gave thirteen series of measurements and also thirteen corresponding series of "ranks" or orders. Correlation coefficients were now evaluated, using both actual measurements and also ranks. In the former case, Pearson's full method was used, the measurements were grouped, correlation tables were drawn up, and the product-moment formula, $r_{xy} = \frac{S(xy)}{n\sigma_x\sigma_y}$, was applied*. In the case of ranks, two methods were used, (1) the product-moment formula, which in the case of ranks reduces to $1 - \frac{6S(d^2)}{n(n^2-1)}$ where d = difference of corresponding ranks in the two series, was applied. This gave what Pearson† calls ρ, or "rank-correlation," and the result was then converted into "true variate correlation" by the formula $r = 2\sin\left(\frac{\pi}{6}\rho\right)$; (2) the formula $R = 1 - \frac{6S(g)}{n^2-1}$, where g = gain in rank of second series on first‡, was applied, and again the true variate correlation was obtained by Pearson's formula

$$r = 2\cos\frac{\pi}{3}(1-R) - 1\S$$

The above-mentioned two formulae giving r in terms of ρ and R respectively were deduced by Pearson on the assumption of "normal" or Gaussian distribution. If the correction is not made, in other words if ranks are themselves taken as measurements, the distribution obtained is a rectangle,—not obviously a reasonable assumption.

The coefficients (crude values) obtained in these three ways are given in the following table:—

	$r=\frac{S(xy)}{n\sigma_x\sigma_y}$ [marks]	$r=2\sin\left(\frac{\pi}{6}\rho\right)$ [ranks]	$r=2\cos\frac{\pi}{3}(1-R)-1$ [ranks]
Arith. Alg. ...	·79 ± ·03	·65	·65
Geom. Alg. ...	·66 ± ·04	·63	·59
Geom. Arith. ...	·58 ± ·05	·61	·57
CD ...	·81 ± ·02	·77	·74
EH ...	·69 ± ·04	·55	·57
CG ...	·59 ± ·05	·56	·53
FI ...	·49 ± ·06	·49	·51
GI ...	·49 ± ·06	·54	·55
DG ...	·44 ± ·06	·43	·46
BG ...	·26 ± ·07	·24	·24

* For a specimen correlation table, see Appendix II.

† Karl Pearson: "On Further Methods of Determining Correlation," *Drapers' Company Research Memoirs*, Biometric Series, IV., London, Dulau & Co., 1907, pp. 11 and 18.

‡ C. Spearman: "'Foot-rule' for measuring Correlation," *Brit. Journ. of Psychology*, Vol. II. Part I, July, 1906, pp. 100—104.

§ Karl Pearson: *Op. cit.* p. 17. Spearman suggested in his "Foot-rule" article the formula $r = \sin\left(\frac{\pi}{2}R\right)$, which he attempts to justify on merely *empirical* grounds. A full criticism of his work on "ranks" will be found in Pearson's article above-mentioned.

The discrepancies between mark-values and grade- (or adjusted rank) values are due to deviations from normal distribution*. This is empirically obvious from the fact that the discrepancies are larger the larger the deviations from normal distribution. This is also the case with the following additional coefficients, calculated by only two of the methods:—

	$r=\frac{S(xy)}{n\sigma_x\sigma_y}$ [marks]	$r=2\cos\frac{\pi}{3}(1-R)-1$ [ranks]
Arith. G ...	·78 ± ·03	·75
Alg. G† ...	·71 ± ·04	·57
Geom. G ...	·67 ± ·04	·60
AC ...	·78 ± ·03	·81
AD ...	·65 ± ·05	·60
FG ...	·64 ± ·05	·63
BC ...	·60 ± ·05	·68
CI ...	·55 ± ·06	·57
CF ...	·51 ± ·06	·50
AB ...	·49 ± ·06	·65
BD ...	·37 ± ·07	·29

Thirty-three other coefficients were worked out by the formula

$$r=2\cos\frac{\pi}{3}(1-R)-1$$

only. They are as follows:—

Geom.	A	·81	HI	·92	AF	·46
,,	B	·55	EF	·82	DE	·46
,,	C	·94	EG	·76	AE	·44
,,	D	·82	AH	·61	DI	·43
Arith.	C	·54	CH	·57	AG	·41
,,	D	·43	EI	·57	DH	·40
,,	E	·91	FH	·55	BF	·28
,,	F	·80	GH	·53	BH	·26
Alg.	C	·59	DF	·49	BE	·23
,,	D	·41	AI	·47	BI	·22
,,	H	·96	CE	·47		
,,	I	·93				

All the above coefficients are "crude," i.e. uncorrected for the effect of extraneous conditions. In the present case the disturbing conditions are principally difference of age and difference of class or form. To correct for the first, coefficients were evaluated between age and each of the other twelve series. For ranks

$$\left[r=2\cos\frac{\pi}{3}(1-R)-1\right]$$

* Considering the smallness of the sample, and therefore the largeness of the random deviations from possible normality, the two formulae lead to results in strikingly close accordance and suggest normal distribution as being the rule for the psychical characters observed.

† The exceptionally large discrepancy here corresponds with the fact that the characters compared have the two most skew distributions of the series, their distributions being: *Alg.* 26, 16, 19, 9·5, 6·5, 4, 1, 1 (a J-curve). *G* 30, 26, 12, 4, 2, 9 (a U-curve). Unit of grouping, 7 marks.

the values were as follows:—

Correlations with Age (ranks).

Geom.	0·19×	B	0·11	F	0·02
Alg.	0·19×	C	0·06	G	−0·12
Arith.	0·04	D	0·05	H	0·18×
A	0·21×	E	−0·02	I	0·19×

$$\text{P.E. for zero correlation} = \frac{\cdot 7738}{\sqrt{83}} = 0{\cdot}09.$$

The only values greater than twice the probable error, and therefore possibly appreciable, are those marked with a cross. The corresponding values, using marks, are:—

Geom. 0·06, Alg. 0·23×, A 0·09, H 0·19×, I 0·07,

$$\text{P.E. for zero correlation} = \frac{\cdot 67449}{\sqrt{83}} = 0{\cdot}075.$$

In the second case, therefore, only Alg. and H are affected appreciably. Owing to the discrepancies between marks and ranks, only the coefficients obtained from marks were corrected. The formula used was, of course, the partial correlation coefficient $\bar{r}_{12}$ for constant age*, viz.

$$\bar{r}_{12} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{(1 - r_{13}^2)(1 - r_{23}^2)}}.$$

Thus, for Geom. Alg., putting Geom. 1, Alg. 2, Age 3,

$$\bar{r}_{12} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{(1 - r_{13}^2)(1 - r_{23}^2)}} = \frac{\cdot 66 - 0 \times \cdot 23}{\sqrt{(1 - 0^2)(1 - \cdot 23^2)}} = \frac{\cdot 66}{\cdot 973} = \cdot 68.$$

The second main disturbing factor was the arrangement of the boys in sets or forms. The boys of the higher sets might have been expected to do better in every part of the work than those of the lower sets, because they had covered more of the syllabus than the others. Instead, however, of calculating correlation coefficients between order of form and each of the series of measurements, the "correlation ratio," η, was evaluated†. In carrying out the correction, η is substituted instead of r in the partial correlation coefficient.

The values of η obtained were the following:—

Correlation Ratios, "Form" and Character.

Geom.	·71	D	·57
Arith.	·59	E	·73
Alg.	·87	F	·59
A	·59	G	·65
B	·28	H	·76
C	·66	I	·80

Age ·41

The fact that η for age is ·41 while the correlation between age and total marks is nil seems to show that the effect of difference of form in the other series is due

* See, e.g., G. Udny Yule: "On the Theory of Correlation," *Journal of the Royal Statistical Society*, Vol. LX. Part IV. Dec. 1897, p. 833.

† See Karl Pearson: Biometric Series II, *Drapers' Company Research Memoirs*, pp. 9 ff. For an example of the method, see Appendix II.

mainly to the difference in the extent of syllabus covered in the different forms. This, at any rate, seems to be the most satisfactory solution of a difficulty which the writer himself would be the first to admit.

The finally corrected values of the correlations are :—

Alg. Arith.	·76 ± ·03	BD	·27 ± ·07	FG	·41 ± ·06
Geom. Arith.	·28 ± ·07	CD	·69 ± ·04	BG	·11 ± ·08
Geom. Alg.	·18 ± ·08	Arith. G	·65 ± ·04	DG	·11 ± ·08
AB	·42 ± ·06	Alg. G	·45 ± ·06	CF	·20 ± ·07
AC	·64 ± ·05	Geom. G	·39 ± ·07	CI	·05 ± ·08
AD	·31 ± ·07	CG	·28 ± ·07	EH	·33 ± ·07
BC	·57 ± ·05	IG	·00 ± ·08	FI	·04 ± ·08

Several interesting results can be obtained from these values by applying the theory of multiple correlation. Thus, limiting our attention for the moment to Geometry, Arithmetic, and Algebra, it is possible for us to deduce the degree of correlation between any two of them on the assumption that the third ability is constant throughout the series. It is given by the formula

$$\bar{r}_{12} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{(1 - r_{13}^2)(1 - r_{23}^2)}}.$$ [See above, p. 359.]

The partial correlation coefficients in the present case are :—

Geom. Alg. $-$ ·05 ± ·08, Geom. Arith. ·23 ± ·07, Alg. Arith. ·75 ± ·03.

We may perhaps infer from these values that geometrical ability is only related to algebraical ability through the mediation of arithmetical ability*.

Taking now four variates, e.g. A, B, C, and D, it is, in a similar way, possible to deduce the degree of correlation between any two, say C and D, on the assumption that the cther two abilities are constant throughout the series. Putting the numbers 1, 2, 3, 4 in the place of A, B, C, D, the partial correlation coefficient

$$\bar{r}_{34} = \frac{r_{34}(1 - r_{12}^2) + r_{31}(r_{12}r_{42} - r_{41}) + r_{32}(r_{41}r_{12} - r_{42})}{\sqrt{1 - r_{12}^2 - r_{14}^2 - r_{24}^2 + 2r_{12}r_{14}r_{24}}\sqrt{1 - r_{12}^2 - r_{13}^2 - r_{23}^2 + 2r_{12}r_{13}r_{23}}} \dagger$$

$$= \mathbf{93}.$$

There is thus a very close relation indeed between memory of propositions in geometry, and the power of recognising general relations in a particular case in geometry.

Obviously, the method could be extended indefinitely.

A glance at the table of corrected coefficients suggests the following additional results :—

(1) the ability to do percentage and proportion sums in arithmetic is more closely related to essential geometrical ability than to essential algebraical ability ;

* This result harmonises with the view that mathematical reasoners fall into two types, the so-called "geometrical" (or "intuitional") and the "analytical" (or "logical") types. See H. Poincaré : *La Valeur de la Science*, pp. 11—15.

† See Karl Pearson : "Selection, etc." *Phil. Trans.* Vol. 200.A, p. 31.

(2) the close relation between arithmetical ability and algebraical ability is due mainly to habits of accuracy common to both;

(3) memory of preceding propositions is the ability most closely related to the other forms of geometrical ability in school-boys,—is, in fact, the central ability in school geometry.

It would be interesting to test the results for the existence of a general geometrical ability or a general mathematical ability, but unfortunately the only formula hitherto suggested for such a purpose, viz. that given by Dr Spearman*, is open to very serious criticism†. The writer hopes to return to the problem in a future paper.

* Krueger and Spearman: *Zeitschrift für Psychologie*, Bd. 44, 1906, p. 84.

† See Clark Wissler: "The Spearman Correlation Formula," *Science*, N. S. Vol. XXII. No. 558, Sept. 8, 1905, pp. 309—311.

William Brown: "Some Experimental Results in Correlation," *Comptes Rendus du VI^me Congrès International de Psychologie*, Genève, Aug. 1909. In this pamphlet mention was made of the assumptions upon which the applicability of Spearman's formula was based, and an alternative proof of the formula based on those assumptions, contributed by Mr G. Udny Yule, was brought forward.

The formula may be written

$$r^2_{XY} = \frac{r_{X_1Y_1} r_{X_1Y_2} r_{X_2Y_1} r_{X_2Y_2}}{r^2_{X_1X_2} r^2_{Y_1Y_2}},$$

where X and Y represent the unknown *true* values to be correlated, and X_1, X_2; Y_1, Y_2 are two pairs of *observed* values, vitiated more or less by errors of observation.

Let
$$x_1 = x + \delta_1, \quad y_1 = y + \epsilon_1,$$
$$x_2 = x + \delta_2, \quad y_2 = y + \epsilon_2,$$

where x, y, δ, ϵ represent deviations from means.

The assumption involved in the formula is that the errors of measurement δ_1, δ_2, ϵ_1, ϵ_2 are uncorrelated with each other or with x or y.

I suggested that the justifiability of this assumption might be tested by correlating $X_1 \sim X_2$ and $Y_1 \sim Y_2$, $X_1 \sim X_2$ and X_1, $Y_1 \sim Y_2$ and Y_1 which should all give a zero value for the coefficient. In this statement the sign of subtraction (−) should have been used instead of the difference-sign (∼), and only the first of the three coefficients, viz. that between $X_1 - X_2$ and $Y_1 - Y_2$, should be expected to give a zero value.

Thus,
$$r_{\substack{X_1 - X_2 \\ Y_1 - Y_2}} = \frac{S\{(x_1 - x_2)(y_1 - y_2)\}}{\sqrt{S(x_1 - x_2)^2 \, S(y_1 - y_2)^2}}$$
$$= \frac{S\{(\delta_1 - \delta_2)(\epsilon_1 - \epsilon_2)\}}{\sqrt{S(\delta_1 - \delta_2)^2 \, S(\epsilon_1 - \epsilon_2)^2}}$$
$$= 0, \text{ since the numerator vanishes.}$$

Applying this test to some of the material on which I based my Geneva pamphlet, I found:—

1. In the case of accuracy in bisecting and trisecting lines, where the subjects of the experiment were 43 adults (Group A in the paper)
$$r_{\substack{B_1 - B_2 \\ T_1 - T_2}} = 0{\cdot}30 \pm 0{\cdot}09;$$

2. In the case of speed (S) and accuracy (A) in the addition of series of 10 single digits, where the subjects were 38 elementary school children, girls between the ages of 11 and 12 (Group C)
$$r_{S_1 - S_2, A_1 - A_2} = 0{\cdot}35 \pm 0{\cdot}09.$$

Thus, in each case a correlation greater than three times its probable error was found, showing that errors *were* almost certainly correlated among themselves and hence that Spearman's formula could not be applied.

In cases, however, where the correlation does work out to zero we cannot infer that errors are not correlated, but only that
$$S(\delta_1\epsilon_1) + S(\delta_2\epsilon_2) = S(\delta_1\epsilon_2) + S(\delta_2\epsilon_1).$$

The formula may still be quite inapplicable.

The nature of the correlation tables, and the form of the regression curves to which they gave rise, seemed to indicate that the number of boys examined (83) was sufficiently large to give fairly reliable results. On the other hand, the numbers furnished by separate school forms of average size (say 20) are certainly too small. Thus, the correlation coefficient between mathematics and classics was calculated for the Mathematical Eighth Form of the same school for Christmas, 1907, and again for Christmas, 1908. The two values were 0·20 and 0·52 respectively, showing a discrepancy of 160 °/₀ of the smaller value! Similarly, the mathematics-classics coefficient for the VIIth, U. VIth, and L. VIth forms of the mathematical division investigated in this paper worked out as 0·23, 0·76, and − 0·25 respectively. This would seem to indicate that the results of Correlational Psychology hitherto obtained, where small numbers of cases have almost invariably been used, are utterly unreliable. In such cases the ordinary formulae for probable errors (the proofs of which rest on the assumption of a large number of observations) do not apply, and consequently no clear appreciation of the significance of the results is possible.

APPENDIX I.

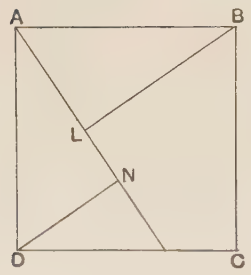
A. *Sample of questions in Geometry.*

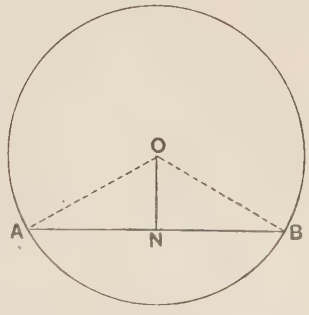
1. (*a*) If two triangles have two angles and a side of one equal respectively to two angles and a side of the other, they will be equal in all respects.

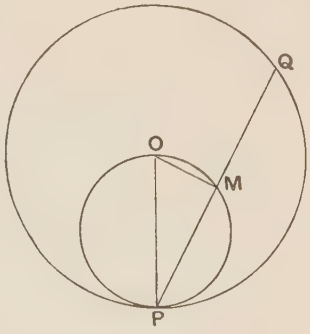
   (*b*) *ABCD* is a square, perpendiculars *BL* and *DN* are drawn to any straight line through *A*. Prove that $AL = DN$.

4. (*a*) The straight line from the centre of a circle, perpendicular to a chord, bisects the chord.

   (*b*) *OP* is the radius of a circle. On *OP* as diameter another circle is described. Prove that all chords of the larger circle drawn through *P* are bisected by the smaller circle.







B. *Sample of analyses of above two Geometry questions.*

1. (*a*) (i) Recognition of necessity of generality in the proof.

(ii) Memory of general truth that three angles of a triangle equal two right-angles, and application to this case.

(iii) Principle of "proof by superposition": memory, sufficient understanding, and application of it to case under consideration.

(*b*) (iv) Power of recognising general relations in a particular case.

(v) Readiness to note and make use of the more special relations obtaining in the particular case.

4. (*a*) (i) Rational or merely visual memory of construction.

(ii) Connecting equality of lines with equality of triangles, of which the lines are sides.

(iii) Memory of result of a particular theorem (on right-angled triangles).

(*b*) (iv) Power of keeping essential and general relation of above theorem in mind, and of looking at the present case through the medium of it.

(v) Memory of theorem on angle in a semi-circle.

(vi) Power of seeing that the relation is a general one (i.e. power of noting from just what facts or relations the conclusion follows, and of realising that the sequence is not influenced by other facts).

C. *Sample of Marking-Sheet. Upper VIth*

Questions.

Name	Age		*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	Totals
C. E. B.	14·0	Geom.	9, 0 10, 10, 9, 0, 0	3 2, 2, 5	—	10, 0 10, 10, 10, 0, 0, 0	10, 15 10, 10, 10, 10, 10, 10	8 10, 2, 0, 5	0, 0 0, 0, 0, 0	—	—	—	—	—	—	55
		Arith.	10 10, 10	10 0, 0, 10	10 10, 10, 10, 10	1 10, 8, 5	10 10, 10, 10, 10	10 10, 10	0 0, 0	4, 3 10, 9, 8	14 10, 10	12 10, 10, 0, 10	14 10, 10	14 10, 10, 10	—	112
		Alg.	7, 8 10, 10, 10	7 10, 10	4, 5, 6 10, 10	0, 8 10, 10, 10, 7	4 8, 6	—	15 10, 10, 10, 10	5 10, 7, 6	15 10, 10	15 10, 10, 10	—	—	—	99

APPENDIX II.

A. *Evaluation of* $r_{\substack{\text{Geom.}\\ \text{Arith.}}}$. *Correlation Table.*

Geometry.

Arithmetic.	0— 15	15— 30	30— 45	45— 60	60— 75	75— 90	90—105	105—120	120—135	Totals
0— 15	—	1	2	1	—	—	—	—	—	4
15— 30	1·5	7·5	3·5	2	—	—	1	—	—	15·5
30— 45	1	7	5·5	7·5	1·5	1	—	—	—	23·5
45— 60	—	2	7	5	7·5	—	1	—	—	22·5
60— 75	—	—	2	0·5	—	2	—	—	1	5·5
75— 90	—	—	1	0·5	—	2	—	1	—	4·5
90—105	—	—	—	2	1·5	1	—	—	1	5·5
105—120	—	—	—	1	—	—	—	—	—	1
120—135	—	—	—	—	—	1	—	—	—	1
Totals	2·5	17·5	21	19·5	10·5	7	2	1	2	83

B. *Evaluation of* η *for Arithmetic and Order of Form.*

Form (x).

Arithmetic (y).	*U. V*^{B}	*U. V*^{A}	*L. VI*	*U. VI*	*VII*	Totals
0— 15	2 2	—	2 2	—	—	4
15— 30	8 1	2 1	3·5 1	2 1	—	15·5
30— 45	5 0	7 0	4·5 0	7 0	—	23·5
45— 60	3 1	4 1	5 1	4 1	6·5 1	22·5
60— 75	—	2 2	1 2	1 2	1·5 2	5·5
75— 90	—	—	1 3	0·5 3	3 3	4·5
90—105	—	—	—	1·5 4	4 4	5·5
105—120	—	—	—	1 5	—	1
120—135	—	—	—	1 6	—	1
Totals (n_x)	18	15	17	18	15	83 (N)
Means (y_x)	−·50	·40	·15	1·36	2·30	$\bar{y}=$ ·68 $\sigma_y^2=2·65$

—Arbitrary mean from which true means and standard deviations are calculated.

$$\eta^2=\frac{S\{n_x(y_x-\bar{y})^2\}}{N\sigma_y^2}=·3577,$$

$$\therefore\ \eta=·59.$$

APPENDIX III. *Marks and Ranks*.*

Form	Name	Age	Rank	Total Marks	Rank	Geom.	Rank	Arith.	Rank	Alg.	Rank	A	Rank	B	Rank	C	Rank	D	Rank	E	Rank	F	Rank	G	Rank	H	Rank	I	Rank
VII. *1*	R. A. A.	16·3	23½	145	28½	55	30	49	32	41	24	40	8	30	37	44	41½	15	50	72	19½	51	58½	17	24	53	11	70	13½
	A. M. J. B.	15·5	44½	220	9	48	39½	93	6	79	4	18	60	30	37	44	41½	9	65	77	15½	88	11	40	3½	62	4½	86	5
	W. T. C.	14·11	55	212	10½	85	8	65	15	62	10	40	8	27	59	92	4	42	11½	80	12	70	30½	25	13½	70	3	87	4
	W. K. C.	14·9	59⅓	253	5	132	1	59	20	62	10	48	2	40	6	127	1	78	2	60	33½	84	14	24	15	53	11	88	3
5	C. K. D.	15·8	36⅓	256	3	125	2	94	5	37	29	50	1	30	37	120	2	79	1	101	5	91	9	37	7½	41	24½	45	26
	H. A. E. J.	17·3	10⅓	168	19½	68	17	52	29	48	18	20	47	40	6	60	21½	30	28	65	25½	87	13	20	19	35	33	57	20
	L. P. F.	16·0	29	178	15	93	5	53	26½	32	35½	30	26	40	6	80	7	60	3	58	36½	80	16½	5	55½	30	40½	33	39
	H. B. H.	14·3	73	159	23	58	26	60	18	41	24	30	26	30	37	60	21½	40	15½	56	40½	93	8	20	19	42	22½	27	47
	A. G. L.	18·6	2	242	6½	81	9	87	9	74	5	30	26	28	58	70	10½	30	28	73	17½	76	20½	40	3½	46	16½	73	11
10	S. S. M.	15·4	46	242	6½	80	10½	96	4	66	8	38	15½	39	12⅓	69	13	30	28	104	2½	108	6	35	9½	58	7½	75	10
	R. H. P.	15·8	36½	187	14	80	10½	80	10	27	43	38	15½	40	6	86	5	37	20	85	10	75	22½	37	7½	33	35½	45	26
	D. W. R. R.	15·7	39½	212	10½	64	19	91	7	57	12	40	8	40	6	63	16	18	46	86	9	105	7	40	3½	53	11	69	15
	A. S.	16·4	20	284	1	111	3	78	11	95	2	39	14	30	37	95	3	58	4	88	8	115	5	40	3½	74	2	100	1
	E. J. S.	14·11	55	154	26	54	34	59	20	41	24	20	47	30	37	60	21½	30	28	77	15½	65	41½	12	28½	32	37½	46	23½
15	A. C. T.	17·10	4½	176	17½	74	13	46	39	56	13	30	26	30	37	50	34	30	28	67	24	71	26	16	26	55	9	77	9
U. VI.	S. M. A.	15·3	48½	102	52	27	68½	35	54½	40	26½	20	47	30	37	35	49	0	75	43	62½	48	60½	10	40½	44	19	56	21½
	O. D. A.	14·4	70	199	13	56	27	90	8	53	14½	30	26	30	37	50	34	48	7½	93	6	121	2	17	24	58	7½	72	12
	C. E. B.	14·0	77	266	2	55	30	112	2	99	1	29	32½	22	62	60	21½	20	39	103	4	117	3	30	11	79	1	98	2
	H. A. D.	15·8	36½	145	28½	59	25	35	54½	51	16	28	34½	40	6	65	14½	45	10	44	60½	43	65	10	40½	46	16½	83	8
20	C. J. F.	16·5	17½	209	12	89	6	71	13	49	17	35	17	35	16½	80	7	48	7½	89	7	116	4	35	9½	50	15	46	23½
	W. F.	15·9	33	136	34	33	58½	41	45½	62	10	12	65	20	67½	32	51	20	39	50	50	66	39	0	72	44	19	84	6½
	M. A. H.	16·11	13½	117	38	39	48	25	69½	53	14½	26	36	20	67½	44	41½	17	48	32	76	40	66½	0	72	43	21	56	21½
	F. R. L.	14·3	73	158	24	96	4	27	67	35	32½	40	8	25	60	80	7	42	11½	50	50	74	24½	27	12	52	13½	66	16
	G. C. M.	16·11	13½	142	30½	60	23	46	39	36	30½	30	26	30	37	54	28	20	39	55	43	70	30½	10	40½	37	28	43	28
25	J. C. T. R.	15·9	33	176	17½	60	23	43	42½	73	6	34	18	30	37	62	17	35	21½	65	25½	68	37½	10	40½	62	4½	84	6½
	J. T. S.	16·5	17⅓	141	32	72	14½	48	34½	21	49½	20	47	30	37	70	10½	50	5½	64	28	65	41½	15	27	20	55	34	37
	C. M. S.	15·3	48½	68	67	17	78½	40	48	11	62½	0	82	10	81	2	81	10	59	70	22	74	24½	20	19	8	75	20	56
	R. M. S.	15·8	36½	254	4	88	7	123	1	43	21½	40	8	30	37	65	14½	40	15½	118	1	128	1	40	3½	35	33	28	46
	H. S.	14·8	62	142	30½	40	46⅔	57	23	45	20	20	47	30	37	50	34	10	59	57	39	70	30½	0	72	36	30	70	13½
30	H. A. T.	16·4	20	86	57	17	78½	33	58	36	30½	10	69½	30	37	20	71	0	75	53	47	38	70½	0	72	28	43½	36	34
	G. P. W.	16·2	26	241	8	60	23	99	3	82	3	40	8	40	6	70	10½	30	28	104	2½	88	11	10	40½	60	6	60	17
	N. S. W.	14·1	75	164	21	66	18	58	22	40	26½	40	8	30	37	55	26⅓	40	15½	72	19½	65	41½	20	19	40	26	58	19
	W. J. W.	16·3	23½	90	55	33	58½	34	56½	23	47½	1	80	20	67½	30	57½	20	39	49	53	44	64	10	40½	22	51	32	41
L. VI.	P. B.	14·0	77	61	73	16	80	38	51½	7	67½	6	77	20	67½	10	78½	0	75	54	45½	69	35½	10	40½	22	51	10	69
35	J. P. C.	16·0	29	77	63	53	37	11	82	13	58½	20	47	20	67½	30	57½	40	15½	33	75	45	63	2	59	21	53½	31	43
	F. S. H. F.	15·2	51½	76	64½	35	53⅓	30	64	11	62½	20	47	30	37	40	45	10	59	34	74	65	41½	0	72	10	70½	18	59½
	S. W. I.	17·11	3	115	40	35	53⅔	48	34½	32	35½	20	47	40	6	30	57½	0	75	58	36½	68	37½	9	52	30	40⅔	32	41
	G. L.	16·6	16	104	51	44	43⅓	31	61½	29	39	33	19½	30	37	40	45	30	28	43	62½	69	35½	0	72	42	22½	42	29
	E. P. M.	17·4	9	132	35	41	45	63	16	28	41½	30	26	30	37	17	74	19	44	73	17½	75	22½	10	40½	44	19	18	59½
40	T. C. M.	17·3	10½	105	49½	55	30	25	69½	25	44½	30	26	30	37	50	34	10	59	48	54	53	56	5	55½	29	42	25	50

	H. L. P.	16·3	23½	65	70½	35	53½	28	66	2	78½	33	19½	32	18	33	50	10	59	44	60½	51	58½	0	72	6	77	4	75½	
45	S. L. R. S.	18·10	1	66	69	34	56	14	80	18	53	20	47	29	56½	30	57½	0	75	18	82	26	78	0	72	26	45½	17	63	
	T. L. S.	14·10	57½	110	44	28	65½	47	37	35	32½	9	74	20	67½	20	71	0	75	60	33½	56	53½	10	40½	31	39	36	34	
	E. G. T.	15·9	33	105	49½	38	49½	46	39	21	49½	18	60	20	67½	20	71	16	49	70	22	70	30½	10	40½	22	51	39	31½	
	S. H. T.	16·4	20	120	37	44	43½	48	34½	28	41½	30	26	30	37	48	39	18	46	40	64½	38	70½	10	40½	33	35½	30	44	
	R. W.	15·6	42	115	40	55	30	40	48	20	51	18	60	19	75	50	34	50	5½	58	36½	61	45	8	53	28	43½	29	45	
50	W. T. W.	17·5	8	153	27	77	12	43	42½	33	34	23	38	30	37	52	29	35	21½	46	57½	80	16½	0	72	25	48	23	52	
U. V.A	W. C. A.	14·7	64½	65	70½	27	68½	26	68	12	60½	19	56½	30	37	22	68	40	15½	47	55½	58	49	0	72	21	53½	20	56	
	K. C. M. A.	14·8	62	83	59	47	41	31	61½	5	72	20	47	30	37	50	34	30	28	35	72½	38	70½	0	72	14	64½	12	68	
	D. I. B.	16·1	27	110	44	53	37	34	56½	23	47½	40	8	38	14½	50	34	30	28	50	50	53	56	0	72	25	45	14	66	
	A. C. H.	15·7	39½	168	19½	53	37	69	14	46	19	20	47	30	37	50	34	20	39	54	45½	57	52	40	3½	36	30	34	37	
55	R. J. J.	15·2	51½	108	46½	40	46½	53	26½	15	57	28	31½	30	37	38	47½	10	59	58	36½	88	11	10	40½	16	59	17	63	
	H. M. L.	14·0	77	108	46½	37	51	62	17	9	64	30	26	35	16½	38	47½	14	51	81	11	79	18	20	19	11	67½	5	73	
	E. J. L.	13·11	80	76	64½	28	65½	19	76	29	39	13	63½	30	37	30	57½	0	75	24	80	23	80	10	40½	35	33	39	31½	
	M. M.	16·7	15	91	54	23	75	44	41	24	46	20	47	19	75	12	76	0	75	61	32	53	56	5	55½	26	45½	32	41	
	F. C. M.	14·6	67	177	16	55	30	53	26½	69	7	20	47	23	61	60	21½	30	28	55	43	63	44	20	19	52	13½	36	34	
60	R. F. N.	15·11	31	89	56	38	49½	48	34½	3	76½	20	47	20	67½	30	57½	10	59	55	43	47	62	10	40½	6	77	3	78	
	C. T. N.	15·3	48½	111	42	54	34	32	59	25	44½	40	8	40	6	60	21½	40	15½	47	55½	56	53½	0	72	25	48	21	53½	
	J. G. P.	17·2	12	78	62	18	76½	31	61½	29	39	0	82	10	81	10	78½	13	52	40	64½	58	49	5	55½	32	37½	34	37	
	N. P. V. P.	15·5	44½	107	48	48	39½	42	44	17	55	19	56½	30	37	40	45	20	39	62	31	82	15	25	13½	15	61½	26	48½	
	F. L. W.	14·9	59½	115	40	71	16	38	51½	6	70	40	8	40	6	55	26½	46	9	50	50	70	30½	2	59	15	61½	4	75½	
65	H. E. W.	17·9	6	130	36	62	20	50	31	18	53	29	32½	38	14½	60	21½	20	39	63	30	70	30½	17	24	15	61½	26	48½	
U. V.B	L. J. B.	13·4	83	55	75	32	62	16	79	7	67½	18	60	30	37	28	65	0	75	36	69½	40	66½	0	72	17	57½	8	71	
	H. B.	13·11	80	50	78	25	72	17	78	8	65	10	69½	30	37	30	57½	0	75	24	80	8	82½	10	40½	14	64½	20	56	
	A. L. C.	15·3	48½	48	79	26	70½	20	74	2	78½	10	69½	20	67½	20	71	10	59	36	69½	35	74	2	59	10	70½	4	75½	
	H. W. D.	14·8	62	138	33	61	21	59	20	18	53	40	8	39	12½	70	10½	38	19	78	13½	70	30½	20	19	17	57½	18	59½	
70	A. C. F.	14·3	73	82	60	46	42	24	71	12	60½	20	47	40	6	50	34	10	59	28	77	28	76	0	72	13	66	13	67	
	L. C. G.	16·0	29	45	80½	12	82	29	65	4	74½	2	79	19	75	0	82½	12	53	35	72½	38	70½	0	72	9	73½	9	70	
	I. H. G.	15·6	42	72	66	33	58½	36	53	3	76½	20	47	29	56½	30	57½	0	75	36	69½	38	70½	0	72	6	77	7	72	
	R. O. G.	14·7	64½	67	68	24	73½	39	50	4	74½	10	69½	30	37	30	57½	0	75	45	59	70	30½	0	72	10	70½	17	63	
	V. G. T. H.	14·4	70	33	82	15	81	18	77	0	81½	10	69½	10	81	10	78½	10	59	37	67	48	60½	0	72	0	81½	0	81	
75	J. T. H.	14·4	70	63	72	32	62	31	61½	0	81½	3	78	19	75	13	75	18	46	50	50	25	79	12	28½	0	81½	0	81	
	B. L.	16·3	23½	53	76½	28	65½	20	74	5	72	13	63½	30	37	30	57½	0	75	27	78	18	81	10	40½	5	79	4	75½	
	A. F. L.	15·1	53	30	83	24	73½	6	83	0	81½	10	69½	30	37	30	57½	0	75	10	83	8	82½	0	72	0	81½	0	81	
	F. G. B. L.	14·6	67	92	53	33	58½	54	24	5	72	8	75½	19	75	30	57½	20	39	70	22	58	49	10	40½	11	67½	21	53½	
	A. M.	14·11	55	45	80½	32	62	13	81	0	81½	10	69½	18	78	25	66½	26	34	24	80	28	76	0	72	0	81½	0	81	
80	A. H. R.	14·6	67	53	76½	26	70½	20	74	7	67½	18	60	30	37	25	66½	0	75	39	66	38	70½	10	40½	9	73½	18	59½	
	H. C. W.	17·7	7	56	74	8	83	41	45½	7	67½	0	82	10	81	0	82½	0	75	46	57½	76	20½	0	72	10	70½	0	81	
	F. B. H. W.	13·9	82	110	44	54	34	40	48	16	56	10	69½	30	37	44	41½	30	28	64	28	58	49	10	40½	18	56	24	51	
83	D. H. B.	13·11	80	84	58	18	76½	53	26½	13	58½	8	75½	10	81	10	78½	0	75	64	28	60	46	10	40½	15	61½	16	65	

Explanation of letter-headings. A. Marks for memory of definitions and general principles (e.g. principle of superposition) in Geometry. B. Memory of constructions (Geom.). C. Memory of preceding propositions and power of applying them (Geom.). D. Recognition of necessity of generality in proof, and power of recognising general relations in a particular case (Geom.). E. Accuracy in Arithmetic. F. General memory of rules and power of applying them (Arith.). G. Power of doing sums in percentage and proportion (Arith.). H. Accuracy in Algebra. I. General memory of rules and power of applying them (Algebra).

* Note the seemingly great bias of marks for even figures, especially for tens, in columns A, B, C, and D. This is due to the fact that in Geometry the part-capacities marked were such that in most cases they either gained the maximum mark, viz. 10, or failed to obtain any marks at all. See Appendix I, C.

DARWINISM, BIOMETRY AND SOME RECENT BIOLOGY. I.

By KARL PEARSON, F.R.S.

(1) The principle of the "survival of the fitter" as the basis of progressive evolution depends upon the co-existence of three factors, (i) the destruction, or elimination as far as reproduction is concerned, of those less fitted to their environment, (ii) the inheritance of the somatic characters on which the fitness depends, and (iii) the absence of any differential fertility. These essential principles of natural selection are frequently overlooked not only by sociologists but by biologists, and whenever they are disregarded, there an inroad of a serious nature has been made into the Darwinian theory of evolution. It is possible to illustrate this point in a variety of ways. Thus Nature in the struggle for existence in wild life, regards and can regard only somatic characters. A somatic character which is not inherited in a greater or lesser degree has no value in the Darwinian theory of evolution. Again, a genetic constitution which does not manifest itself in somatic characters, i.e. is not directly or indirectly correlated with some one or other somatic character, cannot form the subject of Darwinian evolution. To what extent are patent qualities inherited? This must always be the fundamental problem of any Darwinian interpretation of living forms. And—above all in populations where selective mating is small—this problem must and will remain in the first place a statistical problem. The knowledge of an universal physiological law of heredity would immensely aid artificial breeding; it would essentially aid civilized man in the generations to come, when his knowledge dictates and controls his emotional needs*. But from the pure standpoint of Darwinian evolution, the fundamental problem remains the intensity of the transmission of somatic characters in the

* There is not the least doubt that good physique generally and that mental capacity in many cases, can in the present state of human development have sexual attraction; deformity and imbecility produce sexual repulsion. With greater and more widely spread knowledge of genetics, the gametic constitution of an individual as roughly manifested in the average somatic characters of other members of the stock may well come to be a potent emotion-controlling factor of a practically unconscious kind. Even to-day many human beings would, as far as matters of sex are concerned, experience revulsion rather than attraction from an individual whose relatives were physically, mentally or socially defective. There is no doubt that this emotional state will be emphasised the more we are trained from childhood to give the proper relative weights to environment and heredity.

general population of any species. Those who desire to test the efficiency of Darwinian theory must examine nature in the first place from this standpoint.

(2) Again, recent work on evolution seems to have very largely disregarded the direct possibility of testing in wild life, or under artificial conditions, the destruction of the less fit. The correlations between death-rate and environment, and between somatic characters and death-rate, are essentially problems which can be treated by biometric methods, and which are vital to further progress in our study of evolution. It is true that human vital statistics present an immense material for the relationship of death-rate to environment; but they have not hitherto provided the much needed measures of the somatic characters of the selected. What is much needed is a widening of the breeder's outlook,—that he should not only find what is inherited, but that the age at death and the fertility of his stud should be recorded with special relation to the somatic characters of its members, and to marked differences in environment. Light, warmth, food are all in a high degree within his power of differentiation, and his greatest difficulty, environmental growth changes, ought to be surmountable by the use of standard populations. For the purpose of evolutionary study, once the intensity of heredity has been determined, the physiological process of heredity, important as it is for other branches of science, is secondary; the next important step is the correlation of death-rate with somatic characters. I venture to think that more experimental work than has been recently attempted might well be undertaken in this direction.

(3) Lastly I would refer to the third important factor of evolution—the absence of a differential fertility. This point, it seems to me, we again and again overlook. It is extremely hard to believe that fertility could by any mechanism come to be highly correlated with the peculiar type of character fittest to survive in a given environment. The only easy way to suppose it lies in the case of those species in which length of life is at once a measure of fitness and a measure of fertility. We cannot apply this consideration, however, to the case of species which breed only once. If on the other hand fertility were inherited, and were at the same time correlated with other characters, it is difficult to believe that a selective death-rate could perform its functions, except in the case where the maximum of fertility coincided with the optimum of the selected characters. Personally, the truth that natural selection presupposes the practical absence of genetic selection, has only gradually been forced upon me by the discovery of case after case in which there was little or no demonstrable inheritance of fertility. I began with the conception that fertility would be found to be a markedly hereditary character; I expected to find genetic selection masking or even reversing natural selection. Then, as very small or even insensible values came out for man, horse, swine and mice, I have been forced to the conclusion that the smallness of the hereditary factor in fertility is an essential feature of Darwinian evolution. I should be surprised to find a large inheritance of fertility now, just as I should be surprised not to find a large inheritance of any somatic character. And on the

evidence we have at present, I should not expect to find a correlation between fertility and a somatic character; if it does exist in any case, I should anticipate very rapid changes going on in that species.

(4) One of the most recent investigations in this direction is that of Raymond Pearl on the inheritance of fecundity in Barred Plymouth Rocks. His paper on the number of eggs of this race is an extremely interesting one. It is true that he only gives the crude correlations and does not give the data which would be necessary for their correction. Thus he had 31 mothers* and 180 daughters, and some of his "arrays" were probably due to a *single* mother bird, but this cannot be ascertained from his Tables. In working at fertility in man I found exceedingly small values, until correction was made for weighting of the more fertile mother, and then the values appeared to be small but appreciable. If we put aside, however, the small number of mothers used by Pearl and this question of correction, there cannot be the least doubt that Pearl's data show a low, if not zero, intensity of heredity in fertility, thus confirming in poultry what we already know of many mammals, i.e. the absence of inheritance in the case of fertility which seems a necessary foundation of the idea of Darwinian evolution.

Pearl does not take this zero correlation as confirmation of existing results for fertility. He considers it in some way a confirmation of the pure line theory of Johannsen. We are sorry that we cannot in the least agree with him on this point. For if his view were correct, every high correlation would tell against such a theory, which it certainly does not; and in the case of mammals, where the fertility correlations are zero and the body characters highly correlated between parents and offspring, one correlation would refute, another confirm the pure line theory in the same species. Nor again can data taken from measurements of a character highly influenced by environment be in the least conclusive, if they are handled in the manner recently adopted by Jennings, who selected a few individuals with large values of a character and compared their offspring with the general population. There is no security whatever that the individuals selected did not have excess of character owing to environmental and not to hereditary conditions. Such sweeping and dogmatic statements as those made by Jennings that: "The 'standard deviation' and 'coefficient of variation' express in a pure race mere temporary conditions of no consequence in heredity" merely beg the question of what a pure race may be, or else show a sad ignorance of what work has been done on this very point.

The only manner in which it appears to me that an answer can be found to the question which Pearl and Jennings take as now settled—that is to say, the problem of whether or no selection within the individuals descended from a single unit can or cannot produce an effect—is to correlate ancestry with offspring in species in which self-fertilisation or parthenogenesis takes place. If this be done, then the relation of offspring to parent ought to be identical with the relation of

* Nothing short of 500 mothers would really give a convincing result, I think, where the actual correlation is so extremely small. See Pearl, R. and Surface, F. M.: "Data on the Inheritance of Fecundity obtained from the Records of Egg Production of the Daughters of '200-Egg Hens'." *Maine Agricultural Experiment Station, Bulletin*, No. 166, 1909.

offspring to grandparent. Warren and Johannsen himself* are the only investigators who, so far as I am aware, have hitherto provided material in sufficient mass for approximately answering such a question, and what do we find:

Johannsen: *Phaseolus vulgaris.*

	Correlation	Regression
Offspring and parent	·35 ± ·01	·27
Offspring and grandparent ..	·24 ± ·01	·11

Warren: *Daphnia.*

Offspring and parent	·47 ± ·05	·62
Offspring and grandparent ...	·27 ± ·12	·50

Warren: *Hyalopterus.*

Offspring and parent	·40 ± ·03	·47
Offspring and grandparent ...	·24 ± ·04	·25

In every one of these cases, although, as the probable errors show, not based on sufficient numbers to be absolutely final, the parental is higher than the grandparental correlation. The ratio of the two is almost exactly the 3/5 found for the same ratio in mammals with bisexual generation. Pearl and Jennings put such results—and many others of the Biometric School—on one side, without even reference; on what bases? The former on the foundation that fertility is not inherited in hens—an interesting result but one that might be predicted from what we know of mice and swine. The latter because he has failed under the method adopted by him to discover inheritance in *Paramecium*. From that he argues not only to the whole range of possible heredity in Paramecium but apparently in every living form, whatever its mode of reproduction. The fundamental condition of a crucial experiment of this character must be to show (1) on a sample of sufficient size that the character is inherited at all, and (2) that if it be inherited the relationship between offspring and grandparent is as intense as between offspring and parent. Whether the pure line theory be true or not, it cannot be demonstrated either by showing that a fertility correlation is zero, or by the mere *ipse dixit* of Jennings that "'variations' are mere temporary fluctuations, without effect in heredity, so that their relation to evolution is *nil*."

No biometrician asserts that *every* variation is of hereditary value; on the contrary he asserts the very opposite whenever he tables a correlation less than perfect in parthenogenetic reproduction. What he does assert is that on the average such variations form in a measurable degree the mark of a certain associated gametic character. He would not have thought it worth while to try and determine the correlation of gametic and somatic characters† had he believed every individual variation capable of direct inheritance. Nor would his results be in the least upset if—and some theorists in heredity seem coming to that—every individual were in himself a "pure line." He might mildly wonder

* Elise Hanel's results for *Hydra grisea* will be discussed at length below; the character of the selections made prevents full comparison here.

† See *Biometrika*, Vol. VI., pp. 90, 91.

how all these pure lines came into existence in a single local race on any philosophical scheme of evolution; but it would not affect his treatment of inheritance in populations. Why then should he criticise the work of Johannsen, Hanel or Jennings on "pure lines"? Simply because the experiments made by these biologists are in his opinion insufficient to provide a basis for the sweeping inferences drawn from them, and further because they contradict other biological work, which he thinks just as important and valid. The assumption made by the pure linist is that in reproduction from a single individual the same gametic constitution is transferred to every offspring. The biometrician sees no sufficient justification of such a view in our present state of knowledge; two seeds from the same self-fertilised plant may differ in gametic constitution, and this differentiation may appear in the correlated somatic characters. Two Aphides from the same mother, according to Warren, do show such differentiation. Does or does not differentiation follow the division in *Paramecia*? We do not know, because no heredity of the "pure lines" has yet been shown to exist for the character selected by Jennings in the only way that would satisfy a statistician, i.e. a correlation table of the pure line parents with their offspring. On the other hand, having convinced himself that size of litter, for example, is not inherited to any appreciable extent in mice, but that colour and area of colouring are, he is not likely to accept the conclusion of Pearl, based on the non-inheritance of fertility in hens, that "the chief if not the entire function of selection in breeding is to *isolate* pure strains from a mixed population. It is found in actual experience impossible to bring about by selection improvement beyond a point already existing in the pure (isolated) strain at the beginning" (*loc. cit.* p. 79). It is difficult to understand where in the pure strain at the beginning there existed the chief characteristics of any of our domestic animals of to-day. Such statements appear too sweeping in the light of any evidence at present available; they discard the Darwinian spirit of slow and patient inquiry, at the same time that they destroy the Darwinian theory of selective evolution*.

That selection in a *particular direction* cannot go beyond certain limits is of course a commonplace of breeders, indeed it was illustrated at a very early stage by one of the first of biometricians, Sir Francis Galton himself, in his discussion on organic stability. The selection of any one character changes all correlated characters, and a condition is rapidly reached at which the organism is either incompatible with its environment, or possibly its germinal determinants become unstable and assume a new form of equilibrium.

(5) While Jennings himself, as I have said, provides us with no data upon which we can test (i) whether the character he is dealing with is really inherited in *Paramecia*, (ii) whether in an *unselected* population the degree of resemblance of parent to offspring is no greater than that of grandparent—while in fact both Pearl and he appear to confuse non-heredity of a character with pure line heredity

* Pearl, R. and Surface, F. M.: "Is there a cumulative effect of Selection?" *Zeitschrift für induktive Abstammungs- und Vererbungslehre*, Bd. II., S. 257, 1909, answer a very big question from the same egg data.

—yet he has given his approval and sanction on two separate occasions* to a memoir by Elise Hanel† on asexual propagation in *Hydra grisea*. We have thus a very fair test of what his judgment on a statistical proof of "pure line" heredity is worth.

I propose to discuss at some length Hanel's paper, because it seems to me that no definite conclusions as to "pure lines" can be obtained by investigations of this type, until the fundamental features of the research are modified and the observations treated with an adequate theoretical knowledge of statistics. Hanel begins with a very careful investigation of the growth and environmental changes in the character selected—the number of tentacles in *Hydra grisea*. There is general agreement with Parke's results that the number of tentacles changes with age, size, food and place of culture. Differences in these factors can produce very considerable differences in individuals, and differences in the averages of differentially treated groups, which can amount to as much as 0·5 to 0·8 of a tentacle. These are precisely of the order of the average hereditary differences. Thus:

Parent		Offspring	
Number of Individuals	Number of Tentacles	Number of Individuals	Average of Tentacles
9	6	364	6·943
9	7	310	7·296
4	8	166	7·344
4	9	125	7·383

* "Heredity, Variation and Evolution in Protozoa" II., *Proc. Amer. Phil. Soc.* Vol. XLVII. 1908, p. 521, Jennings definitely asserts that Johannsen in 1903 showed that in beans and barley selection within the pure line had no effect. He then proceeds to state that Hanel has found the same state of affairs—no selection within the pure line will produce an effect—in Hydra. In a second very dogmatic paper "Heredity and Variation in the Simplest Organisms," *The American Naturalist*, Vol. XLIII. 1909, p. 332, Jennings writes: "Work with 'pure lines'—where no intercrossing of races or individuals occurs—is possible with few organisms, and little of it has been done. In the few investigations carried on in this way, the same conditions have been found that we have set forth above for Paramecium. They were first shown by Johannsen to hold for beans and barley, and later by Elise Hanel for Hydra. The fact that there exist diverse races, tending to breed true, has of course been shown for many species, but in most cases it is difficult to maintain pure lines, and thus to *absolutely* demonstrate the relations above set forth, as has been done for beans, barley, Hydra and Paramecium." The italics are mine. He also cites Barber "On Heredity in certain Micro-Organisms," *The Kansas University Science Bulletin*, IV. 1907, pp. 1—48. Now several comments are needed here. Jennings directly overlooks the researches of the Biometric school, which was first to work in the field of pure lines, and actually found (see Warren's papers, *R. S. Proc.* Vol. 65, 1899; *Biometrika*, Vol. I. p. 128, 1901) that variations within the line *were* inherited. The same result appears to flow from Johannsen's and Hanel's own experiments, but the characters they have chosen are so influenced by environment that they have not observed the fact. It does not flow from Jennings' or Barber's experiments because they have not demonstrated that heredity exists at all for the characters selected. In fact Jennings repeatedly (and Hanel also) illustrates how markedly environment dominates the characters taken as a measure of heredity. Jennings then assumes that by selecting a character, the heredity of which he has never demonstrated, he can reach a general and "absolute demonstration" of the truth of the theory of pure lines!

† "Vererbung bei ungeschlechtlicher Fortpflanzung von Hydra grisea," *Jenaische Zeitschrift*, Bd. XLIII. 1908, pp. 322—372.

It will be at once recognised that the differences here are rather less than many of the environmental differences, and there is no security whatever that these 26 foundation stock Hydra are really represented by differentiated hereditary numbers of tentacles. Yet this table as it stands embraces Hanel's proof that number of tentacles is an hereditary character in the "pure line." What evidence is there that any one of the numbers of tentacles attached to those 26 parents is truly constitutional and not environmental? In every generation that follows Hanel selects from each line two or three individuals to be parents of the following generation. In selecting a few isolated individuals in each generation, where non-hereditary influences are so influential, we may break the effect of heredity at each step, and since such influences are equally effective with heredity, the chances are that we shall do so once in every *two* selections. Only by taking large numbers of the high and large numbers of the low, would it have been possible to average out the effect of environmental changes. When the line is carried through two or three selected individuals only, there is no certainty that the non-hereditary variations will average out. Of course this statement proceeds on the assumption that heredity is small in this case compared with other factors. Table I gives

TABLE I.

Number of Tentacles of Stem Parent and Offspring.

Number of Tentacles of Offspring.	Number of Tentacles of Stem Parent.								
	5	*6*	*7*	*8*	*9*	*10*	*11*	*12*	Totals
5	1	—	—	—	—	—	—	—	1
6	24	81	54	25	12	1	—	2	199
7	70	139	136	75	36	8	—	8	472
8	23	44	93	50	22	1	—	14	247
9	4	4	23	14	8	6	—	—	59
10	—	—	4	1	2	3	—	1	11
11	—	—	—	1	—	—	—	—	1
Totals	122	268	310	166	80	19	—	25	990

Hanel's data for the 26 original parents and their immediate offspring*. The statistical constants obtained are†:

	Parents (26)	Offspring (990)	Correlation	Regression
Mean	6·996	7·201	·230	·138
Standard Deviation	1·435	·862	±·011	±·013

* A protest must here be raised against the manner in which this author's data are tabled. No proper description is given of the tables, and it is quite impossible to pick out the pedigree of any individual beyond the parent. Further, instead of the frequencies of offspring with each number of tentacles being given, the total number of offspring is stated and the percentages with each number of tentacles. A laborious process had therefore to be undertaken to replace Hanel percentages by the original frequencies. It was then found that many of this author's percentages must be wrongly calculated, for their totals came to 111, 93, etc., instead of 100, while in other cases it was impossible to determine where one or more individuals should be placed on the basis of the percentages given. It is impossible to make anything of a return in which 13 individuals are said to occur in percentages of 15, 31, 40 and 8! On this account 21 families had to be dropped, out of 245 for which data are provided.

† I have to thank most heartily Miss Ethel M. Elderton for the whole of the arithmetical reductions of Hanel's data.

Now these results bring out several interesting points. It is doubtful whether the probable error of the correlation ought to be calculated on 990 cases and not rather on the number of parents, 26. Further, these parents are far from a random sample; the S.D. as compared with that of the offspring shows that the parents have been selected with markedly high and low numbers of tentacles. The regression has thus more meaning than the correlation*. We see then that tentacles in Hydra are really inherited, but with a value ·138, only about a third to a fourth of the intensity of heredity in many other characters in other species—compare Warren's values in the case of *Daphnia* and *Hyalopterus*. Let us see exactly what this means: it signifies that if we were to take a species in which heredity was as marked as in most characters in man, or as marked as in Daphnia, we should have to mix it with two to three times as many pairs in which there was no resemblance between the character in order to weaken down the measure of hereditary resemblance to that manifested by Hanel's *Hydra*. In other words, a mixture in which one-third only exhibited normal hereditary values of the character and two-thirds characters having no hereditary significance, would produce the same intensity of heredity as is manifested in this material†. What chance, we may ask, has any selection of two or three individuals at most out of each family of 20 to 40, got of exhibiting continuous selective influence?

To test the generality of the above results a Table was now formed (Table II) giving the correlation between all available parents and their offspring. We find:

	Parents (224)	Offspring (6436)	Correlation	Regression
Mean	7·234	7·275	·182	·135
Standard Deviation	1·225	·908	±·008	±·006

Thus we clearly see when we pass from the 26 foundation stock to the available 224 parents, that the material has not been so markedly selected, but parents are

* The regression would not be changed by the selection of parents, and would represent the correlation in a stable population.

† The low value of the correlation between parent and offspring is undoubtedly due in chief part to the variation with growth in the number of tentacles. This point has been demonstrated by Albrecht Hase in an interesting memoir ("Ueber die deutschen Süsswasser-Polypen Hydra," *Archiv für Rassen- und Gesellschafts-Biologie*, Jg. VI. S. 721). He shows that the average number of tentacles increases in *Hydra grisea* from the first day of separation as follows:

1st Day	20th Day	50th Day	End of Culture
6·14	6·71	8·42	9·14

It is clear that the hereditary factor must be obscured by such growth changes. But I do not think Hase is correct in his conclusion that the number of tentacles has *no* hereditary value; my analysis shows that the hereditary value is there, but much obscured. Hase holds that Hanel's experiments have shown complete regression in the line, because she is dealing with a character which has no "Erblichkeitswert," and that she has only thus obtained results in accordance with Johannsen's. I am entirely in agreement with him, that a first condition of any experimenting on "pure line" material, is to show that the character chosen has "Erblichkeitswert." This both Pearl and Jennings have failed to do. But Hanel's data do exhibit "Erblichkeitswert," if only in a very attenuated degree. But to the same degree they show, what Hanel and Jennings imagine they do not show, that regression does take place within the pure line, and that properly instituted selection would produce effect within the limits of the pure line. There is a similar attenuated "Erblichkeitswert" in Johannsen's own material which shows in a similar manner to Hanel a partial regression within the pure line.

still somewhat selected material as compared with the offspring. Taking, however, the regression coefficient as less influenced by such selection, we see that the intensity of heredity is essentially the same for all parents as for the foundation stock parents, i.e. ·135 and ·138.

TABLE II.

Number of Tentacles of Parent and Offspring.

Number of Tentacles of Offspring.	Number of Tentacles of Parent.								
	5	*6*	*7*	*8*	*9*	*10*	*11*	*12*	Totals
5	1	—	—	—	—	—	—	—	1
6	38	436	440	290	81	20	5	8	1318
7	86	862	875	724	254	81	5	20	2907
8	33	403	501	493	184	67	4	20	1705
9	4	75	114	134	62	25	—	11	425
10	—	—	10	13	13	13	—	1	50
11	—	—	2	12	9	7	—	—	30
Totals	162	1776	1942	1666	603	213	14	60	6436

We have now to consider the next stage, which presents considerable difficulty, for Hanel has not preserved all the offspring in order to reach a random sample of the second generation, but preserved two or three from each group only. We took

TABLE III.

Number of Tentacles of Stem Grandparent and Offspring.

Number of Tentacles of Offspring.	Number of Tentacles of Stem Grandparent.								
	5	*6*	*7*	*8*	*9*	*10*	*11*	*12*	Totals
6	15	149	157	47	28	14	—	18	428
7	61	243	249	149	92	10	—	26	830
8	44	116	121	124	66	6	—	22	499
9	14	32	29	22	15	2	—	1	115
10	—	—	4	2	1	1	—	—	8
11	—	—	4	—	—	—	—	—	4
Totals	134	540	564	344	202	33	—	67	1884

however all the "grandchildren" of the original foundation stock with the following result:

	Grandparents (26)	Offspring (1884)	Correlation
Mean	7·199	7·181	·030
Standard Deviation	1·288	·884	±·016

Now on the pure line theory the resemblance of the offspring to the parent ought to be no greater than it is to the grandparent. It is here more than *four times as great.* Another very good test of the "pure line," it occurred to me, might further be used to illustrate this point. If the theory of the pure line hold for *Hydra,* the relation of the offspring to the whole fraternity of which the parent

is a member should be as close as to the parent itself—either equally represents the character of the pure line. This method, as far as I can see, might be very profitably used to test pure line theories, because the parent will generally have been reared in the same environment as its co-fraternity*. Table IV has then

TABLE IV.

Number of Tentacles of Member of Parents' Fraternity† and of Offspring of Parents.

Number of Tentacles of Offspring.	Number of Tentacles of Parents' Brethren.							
	5	*6*	*7*	*8*	*9*	*10*	*11*	Totals
6	4	3824	7681	3143	384	70	8	15114
7	15	6370	17869	7980	1587	218	25	34064
8	9	3322	10258	5919	1102	175	22	21807
9	6	733	2514	1235	338	52	8	3886
10	—	36	167	93	35	6	—	337
11	—	5	91	4	11	2	—	154
Totals	34	14290	38580	18415	3457	523	63	75362

been formed giving the relation between offspring and each member of the parents' co-fraternity. We have:

	"Aunt or Uncle"	"Nephew or Niece" (75362)	Correlation	Regression
Mean	7·170	7·213	·091	·095
Standard Deviation	·816	·851	±·002	±·003

It is clear that the correlation between parent and offspring is *double* what it is between the parents' co-fraternity and the offspring, a result wholly inexplicable on the theory of the pure line.

We can follow this idea further and remark that the degree of resemblance between every member of the pure line ought to be exactly the *same*. There ought to be no distinction at all between the correlation of individuals with their ascendants, their descendants and their collaterals in all grades. Now in dealing with man and with mammals generally, the biometric school has found the resemblance of brethren to be slightly greater than that of parent and offspring, parental correlations running from ·4 to ·5 and fraternal from ·5 to ·6. For

* I do not see further why it should not be applied to test "pure lines" in the case of bisexual reproduction, where it is met at once by the fact that the relationship between uncle or aunt and nephew or niece has only about one *half* the intensity of that between parent and child; yet the parent and the uncle or aunt are both products of the same pair of "pure lines." If it be said, which I am ready to admit, that the gametes of one individual are not all alike, that on bisexual reproduction the zygote will not always be the same, and that the somatic characters will be related to these selected gametic characters, the obvious reply is that there is no evidence at present that, either in budding or in parthenogenetic reproduction, there is always a transfer of precisely the same type of cells as basis for the new individual.

† Parent excluded, i.e. "Uncle and Nephew."

greyhounds, Barrington, Lee and Pearson found the ratio of parental to fraternal correlations to be ·466 to ·529, for the horse ·522 to ·633, for man ·460 to ·519; and

TABLE V.

Number of Tentacles of Pairs of "Brethren" (Siblings).

Number of Tentacles of First Sibling.

Number of Tentacles of Second Sibling.	5	6	7	8	9	10	11	Totals
5	—	5	23	11	3	—	—	42
6	5	1878	3599	1426	231	28	7	7174
7	23	3599	8968	4473	941	114	20	18138
8	11	1426	4473	2880	723	107	12	9632
9	3	231	941	723	254	63	7	2222
10	—	28	114	107	63	10	1	323
11	—	7	20	12	7	1	—	47
Totals	42	7174	18138	9632	2222	323	47	37578

Barrington and Pearson for shorthorns ·40 to ·53. Again Warren found for mean results for Hyalopterus and Daphnia ·500 to ·656. It will accordingly be clear that the ratio of fraternal to parental correlation lies between 1·1 and 1·3. If we suppose the weakening of heredity to be as great in the filial as the parental generation, we should expect fraternal resemblance in Hydra to lie between 1·1 × ·136 and 1·3 × ·136, or say between ·15 and ·18. Our results are:

(37,578) Members of same Co-fraternity*		Correlation
Mean	7·212	·167 ± ·003
Standard Deviation ...	·861	

The correlation of "brethren" is thus significantly greater than the corrected correlation, ·135, of parent and offspring, the actual ratio being 1·23. It differs also significantly from the relation between "uncle" and "nephew," the ratio in that case being 1·76, which compares closely with the value for man ·50 to ·30, i.e. about 1·67. It appears impossible therefore, from the three tests made (i.e. comparison of the parental correlation with the correlations of individuals with their grandparents, with their "uncles or aunts" and with their brethren) not to admit that the individual has a special relation to each grade of its line, and is *not* equally related to all members of its line.

Of the results we have reached that for the foundation stock and their grandchildren (·03) is, perhaps, the least satisfactory; it is so low. But the source of this is fairly obvious. Out of each array of offspring of the 26 original Hydra, one, two or three, in one case only, four, individuals were selected to carry on the line. These individuals were not selected at random, nor were they taken proportionally to the frequency distribution of the tentacles in the offspring. The result is that

* Pairs of "brethren" in the first generation.

the grandchildren as they stand are not random samples of the grandchildren which would be due to the original individual. They are arbitrary and not properly representative groups. Thus the correlation of the foundation stock of 26 with their 1452 great-grandchildren is ·059, greater than the value, ·030, of the same individuals' correlation with their grandchildren. It may readily take any small value, because the grandchildren and great-grandchildren are not typical of the stock of the foundation individuals, there having been either one or two selections of two or three individuals of a quite arbitrary character in between. While therefore the correlations for these more distant generations are lower than in the case of parent and offspring, they are quite erratic, and I see no obvious method of allowing theoretically for these arbitrary intermediate selections of parents. It is safest to base the fundamental criticism on the result that the correlation of brethren is sensibly higher, and of "aunts and uncles" very sensibly lower than that of parents and offspring, a result absolutely opposed to the theory of the pure line.

(6) But if those who lightly accept without criticism such results as those of Hanel have not the statistical training necessary to measure degree of resemblance, they might at least have examined Hanel's own tables carefully, and compared the average number of tentacles in the offspring of the parents possessing the highest and lowest numbers of tentacles which occur in each pure line. They would have found the following results:

"Stammthier"	Line	Lowest Number of Parental Tentacles	Number of Offspring	Mean	Highest Number of Parental Tentacles	Number of Offspring	Mean
6	1	6	83	6·82	7	65	7·04
6	2[a]	6	66	6·84	8	13	7·46
5	2[b]	5	30	6·70	8	29	7·34
8	3	6	143	7·42	8	199	7·43
7	4	7	93	7·60	9	64	7·62
6	5	6	202	6·69	8	77	7·15
9	6	6	183	7·06	12	35	7·69
7	7	6	32	7·03	10	31	7·66
7	8	7	100	6·67	8	23	6·78
8	9	6	22	7·32	8	101	7·41
8	10	6	39	7·39	8	116	7·22
6	11	5	39	6·90	7	85	7·11
6	12	6	154	6·97	8	83	7·05
7	13	6	89	6·35	8	50	6·36
7	14	6	24	6·50	8	18	6·61
8	15	7	21	6·93	9	56	7·73
12	16	8	84	7·08	12	25	7·64
10	17	8	31	8·23	10	116	8·45
7	18	6	119	7·34	9	49	7·35
5	19	5	49	7·06	9	26	7·30
7	20	6	95	7·47	9	18	7·44
6	21	6	103	7·49	8	39	8·11
7	22	7	109	7·67	10	23	7·12
5	23	5	44	7·22	9	85	7·40
7	25	7	98	8·05	10	60	7·63
9	26	7	94	7·17	9	78	7·25

The above Table has been prepared from Hanel's means, because these are what Jennings had before him. Many of these means are, I have found, erroneous to a greater or less extent. Further, in a number of cases Hanel did not preserve as parents individuals with the highest and lowest number of tentacles occurring in the "pure line." But what do we perceive even in the data thus presented? *That although the character is very slightly inherited and largely the product of other factors than heredity, yet if we reproduce from the individuals in each pure line with* (a) *the lowest and* (b) *the highest number of tentacles available, the produce of the latter class has in* 22 *out of the* 26 *"pure lines" more tentacles than the produce of the former class.*

In other words, so far from Hanel's observations demonstrating as that author and Jennings suppose that:

(1) "In einer Population von Hydra ist die Selektion wirksam, innerhalb der reinen Linien ist sie ganz ohne Einfluss.

(2) Im Falle 1 ist die Regression eine teilweise, im 2 Falle ist sie vollständig,"

they actually show that regression exists within the "pure line," and that as far as Hydra is concerned the idly termed "fluctuating variations" have a real selective value. The "erfreuliche Uebereinstimmung mit Johannsen's schönen Resultaten," which Hanel says the experiments indicate, is real, but far different in its bearing from what that author imagines. The *Phaseolus* experiments of 1903 and the *Hydra* experiments of 1908 both alike indicate that the offspring are closer to the immediate parent than to other members of the same "pure line." Yet they have both alike been accepted without criticism by distinguished biologists who, suffering from "neo-chytophilia," seem peculiarly anxious to remove the only philosophical basis of Darwinism as a memorial of its fiftieth anniversary.

(7) Let us recognise fully what the "pure line" theory leads to in the case of any self-reproducing organism. It assumes that the cells of an individual destined for the production of new organisms are all alike in character and that variety in the somatic characters of such new organisms, when resulting from a single individual, is not due to any differentiation in the groups of the cells from which the fraternal individuals arise. Every self-reproducing species must have originated with an indefinite number of pure lines—differentiation could not have been produced by selection—or else, differentiation must have been produced at some epoch by what has been termed a fit of mutation. Either solution, the initial indefinite differentiation of a species, or the epochs of mutation, appears to me to destroy Darwinism as a consistent logical system. What then? Simply, that if Darwinism be proved to be in error it must be dropped. But the proofs which are being accepted to-day, that it is in error, are no proofs at all. They are hurried conclusions drawn from observations, which, when treated by any adequate logic, demonstrate—even on ill-suited material—the very reverse of what

they are assumed to prove. I fully appreciate the careful experimental work done by both Jennings and Hanel; I am not pledged to any theory of life, or of inheritance; it is the method and logic of their reasoning which concerns me in the first place. These are essentially inadequate and do not meet the points really at issue. That the degree of resemblance decreases in a geometrical progression, i.e. that offspring are more like their parents than their grandparents or great-grandparents, is not for me a theory, it is a deduction from the observed facts; it becomes a theory, which flows from an hypothesis, if you accept, say Mendelism*. Those facts would not be changed, if the theory of pure lines were demonstrated for either *Paramecium* or *Hydra*. But the establishment of that conclusion would affect biometricians in the indirect manner that any fundamental subversal of Darwinian principles destroys the only philosophical theory of life at present available, and so lessens the interest many feel in devoting time and energy to the measurement of life.

(8) As I have taken Hanel to illustrate recent biological work on the inheritance of variation, I will consider in this section another biological memoir, one on fertility.

I have hitherto made no reference to a recent interesting paper† by Captain R. E. Lloyd, I.M.S. He states that he was led by some observations of mine on the fertility of Shirley poppies to question whether fertility was related to weight in rats. He gives two correlation tables which I reproduce below, but he makes no statistical reduction of these tables, but draws apparently from the mere examination of them the conclusion that gigantic and dwarf rats are just as fertile as common rats of average size. "There is clear evidence that the largest and smallest rats are *quite as fertile as those of average size*" (p. 264).

Now the first impression I formed from Lloyd's tables before they were reduced was that they showed quite sensible correlation between weight of rat and number of young. And this on reduction proved to be the case, the dwarf rats have fewer offspring than the average rat, and the large rats have more offspring than the

* A curiously ignorant account of the biometric treatment of heredity has recently been given by W. Weinberg: "Ueber Vererbungsgesetze beim Menschen," *Zeitschrift für induktive Abstammungs- und Vererbungslehre*, Bd. I. 1909, S. 377 *et seq.* He does not appear while writing his paper to have known the difference between the correlation and regression coefficients; and only when penning the last paragraph did a vague inkling of the difference come to him—through the writings of Johannsen! He overlooks entirely the allowances for assortative mating in biometric work, and accuses the biometric school of neglecting environment, when at any rate in the case of man it is the only group, which by statistical method and by direct investigation has endeavoured to allow for it. Weinberg has stated that the correlation between parents and offspring observed by biometricians in the case of the phthisical diathesis is due to a mixture of classes having different tuberculosis death-rates; and this regardless of the fact that (i) the data of the biometricians were purposely drawn from very uniform classes, and (ii) the correlation of husband and wife for the same material was shown to be much smaller than that of blood relations and in some cases zero. It hardly seems needful to reply to criticisms of this character.

† "The Relation between Fertility and Normality in Rats," *Records of the Indian Museum*, Vol. III. Part III. pp. 261—5.

TABLE VI.

Poona Rats.

Number of Offspring.

Weight in grs. of Mother.	1	2	3	4	5	6	7	8	9	10	11	12	Totals
50	—	—	—	1	3	1	—		—	—	—	—	5
60	—	—	—	1	6	2	—	—	—	—	—	—	9
70	—	—	2	10	17	12	4	—	1	—	—	—	46
80	1	1	11	8	18	10	9	3	2	—	—	—	63
90	2	5	7	18	30	28	12	5	1	—	—	—	108
100	3	5	10	25	37	35	21	7	2	1	—	—	146
110	1	4	12	19	38	37	29	6	2	—	—	—	148
120	2	6	9	21	36	26	30	14	6	—	1	—	151
130	4	4	9	12	35	29	17	17	6	1	1	1	136
140	1	4	6	9	12	27	15	6	2	1	—	—	83
150	—	3	—	2	13	11	6	6	2	—	—	—	43
160	—	2	—	1	11	11	9	3	4	—	—	—	41
170	1	—	1	1	2	4	2	2	1	—	1	—	15
180	—	—	1	1	—	2	2	2	—	—	—	—	8
190	—	—	—	—	—	—	—	—	—	1	—	—	1
Totals	15	34	68	129	258	235	156	71	29	4	3	1	1003

TABLE VII.

Belgaum Rats.

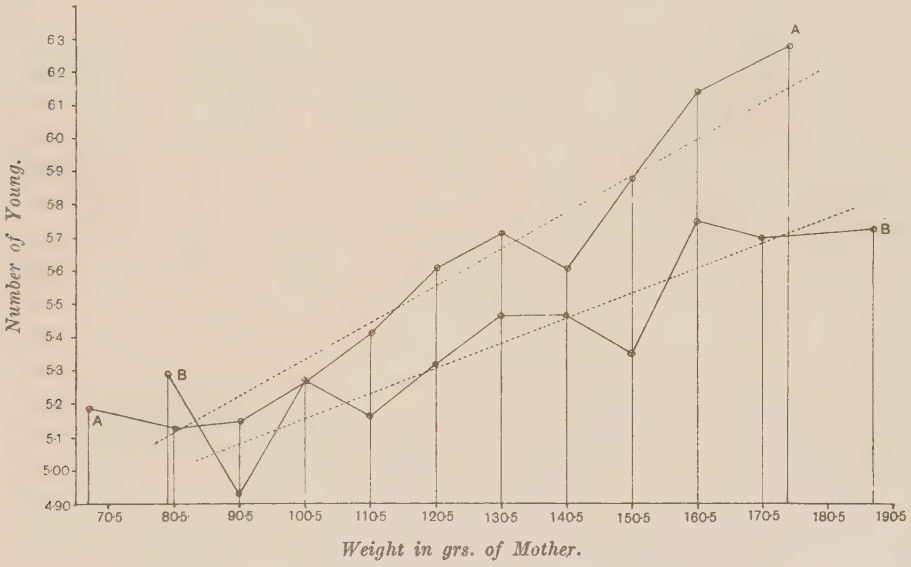
Number of Offspring.

Weight in grs. of Mother.	1	2	3	4	5	6	7	8	9	10	Totals
70	—		—	—	1	1		—		—	2
80	—		—	10	7	5	3	—		—	25
90	—	1	3	13	27	11	2	1	—	—	58
100	2	1	1	18	43	23	14	3	—	—	105
110	—	2	7	23	56	25	15	2	1	—	131
120	2	2	7	23	54	40	23	1	3	—	155
130	1	—	5	18	46	30	16	4	4	—	124
140	2	3	7	15	39	44	18	6	3	—	137
150	—	1	5	20	41	38	10	3	1	1	120
160	—	2	2	4	20	18	7	7	2	—	62
170	—	1	1	4	14	12	9	2	1	—	44
180	—	—	—	3	9	5	4	4	—	—	25
190	—	—	—	1	2	4	1	1	—	—	9
200	—	—	1	2	1	—	2	—	—	—	6
210	—	—	—	—	—	—	3	—	—	—	3
Totals	7	13	39	154	360	256	127	34	15	1	1006

average rat. If w stands for weight, f for number of young, then we have the following results:

	1003 Poona Rats	1006 Belgaum Rats
Mean Weight $\bar{w}$	=113·92	128·84
Mean Number of Young $\bar{f}$ =	5·49	5·38
σ_w =	24·86	25·35
σ_f =	1·68	1·29
r_{wf} =	·1670	·1498
η =	·1733	·1634

As the above results indicate, the regression curves of fertility on weight are almost linear, and the average number of offspring increase at an almost uniform rate from the dwarf to the giant rats. This is well illustrated on the accompanying diagram. It will be seen at once that Captain Lloyd's view is not supported



Regression Curves of Number of Young on Mother's Weight in Rats.
AA Poona Rats. BB Belgaum Rats.

by his own data. There is a very sensible relationship between the weight of the rat and the number of its offspring. But it is not a relation in which the type rat is the most fertile. In other words the case seems at first sight precisely one in which the effect of reproductive selection would cause, without regard to natural selection, a progressive increase in the size of rats. But is this inference really justified? It seems so very important to determine whether any such cases of genetic selection actually occur that we may be pardoned for hoping that further information may be forthcoming. It is conceivable that in certain stages of pregnancy even the size of the litter might have some effect on the weight, but this is hardly likely to produce *all* the difference noted. Another important point

is the question of age. Were the bigger rats on the average the older, and do the older rats have on the average larger litters? If anything of this kind takes place in rats, we should not be comparing the fertility of large and small rats, but the fertility of rats in different stages of development, which is a very diverse point. I am inclined to think that the real relation between number of young and weight of rat, although overlooked by Captain Lloyd, actually has this simple origin. It is suggested, but not demonstrated by the relation which is manifest in mice between size of litter and number of litter. Thus in 700 litters of mice recorded by the late Professor Weldon, I find:

	Number	Average size
First litter	307	5·46
Second „	254	5·57
Third „	139	5·76

It will be seen that the size of the litter increased uniformly with the increasing age of the mouse, and there is little doubt that the weight of the mouse also increased with age. Only a direct experiment in which age and number of litter were taken into account could settle the point. In Captain Lloyd's experimental weighing, there was no record of these characters. I have small doubt that the very sensible correlation between weight and number of young shown by his data is only a secondary result of the relations between age and weight, and again between age and number of young. It is not evidence of a real correlation between fertility and a somatic character, and thus has no bearing on genetic selection, and does not oppose the general principle that fertility is not highly correlated with inherited characters.

(9) The reader may ask: Why have I considered in the same paper such diverse work as that of Jennings, Hanel and Lloyd? The reason lies in this: We have been told recently in an *ex cathedra* fashion by a distinguished biologist* that:

"Of the so-called investigations of heredity pursued by extensions of Galton's non-analytical method and promoted by Professor Pearson and the English Biometrical School it is now scarcely necessary to speak.....A preliminary acquaintance with the natural history of heredity and variation was sufficient to throw doubt on the foundations of these elaborate researches. To those who hereafter may study this episode in the history of biological science, it will appear inexplicable that work so unsound in construction should have been respectfully received by the scientific world."

It seems therefore, however regrettable, needful to re-emphasise the point from which this Journal started more than eight years ago. Biology requires the help of a more exact logic than it appears to possess, above all of an adequate statistical theory to enable it to interpret its observations and test its own theories. I have

* W. Bateson: *Mendel's Principles of Heredity*, Cambridge, 1909, pp. 6—7. Luckily the doctrine of infallibility is not yet accepted in the world of science.

endeavoured to illustrate this in the present paper, by indicating how a want of statistical insight has resulted in at least two cases where experiments have been interpreted in a sense, which is in no way justified by the observations themselves. Regression within the pure line exists in Hydra, and fertility is associated with weight in rats. These are far from the only cases in which principles bearing on evolution have been propounded on the basis of researches, which prove in fact their negation. The biometrician does not desire to waste the energies that may be given to better work in personal recriminations with anonymous writers, nor in controversy with biologists of repute; but it is desirable to assert occasionally that biometry is very far from moribund, and that biology stands to-day even more in need of its methods than it did eight years ago; for it has now developed theories of such complexity, that without the aid of the highest mathematical analysis, it is wholly unable to state whether its theories are accurate or not. And this many Mendelians admit to-day, and all will have to admit in the very near future. From small fragments our Frankenstein has created his soulless monster; he will need all the mathematical aid he can enlist, if his creation is to be brought under control, and not to end in destroying him. But I propose to deal further with some recent Mendelian developments on another occasion.

ADDITIONAL TABLES AND DIAGRAM FOR THE DETERMINATION OF THE ERRORS OF TYPE OF FREQUENCY DISTRIBUTIONS.

SECOND PAPER.

By A. RHIND, B.Sc.

In *Biometrika*, Vol. VII. page 131, a diagram was given to determine the type to which an observed frequency distribution belongs for given values of β_1 and β_2.

The probable errors of β_1 and β_2 define a region within which the most probable values lie.

Since, however, β_1 and β_2 are highly correlated this area is not a rectangle but an ellipse; a contour of the surface

$$z = \frac{N}{2\pi\sigma_{\beta_1}\sigma_{\beta_2}\sqrt{1-R^2_{\beta_1\beta_2}}}\, e^{-\frac{1}{2}\frac{1}{1-R^2_{\beta_1\beta_2}}\left\{\frac{x^2}{\sigma^2_{\beta_1}} - \frac{2R_{\beta_1\beta_2}xy}{\sigma_{\beta_1}\sigma_{\beta_2}} + \frac{y^2}{\sigma^2_{\beta_2}}\right\}},$$

or, referred to principal axes,

$$z = \frac{N}{2\pi\Sigma_1\Sigma_2}\, e^{-\frac{1}{2}\left\{\frac{X^2}{\Sigma_1^2} + \frac{Y^2}{\Sigma_2^2}\right\}} \qquad \text{(A)}.$$

The semi-axes of an elliptic cylinder, with section similar to a contour of (A), and containing half the frequency, are $1{\cdot}177\Sigma_1$ and $1{\cdot}177\Sigma_2$.

These are inclined to the axis of β_2 at angles θ given by

$$\tan 2\theta = \frac{2R_{\beta_1\beta_2}/\sigma_{\beta_1}\sigma_{\beta_2}}{\frac{1}{\sigma^2_{\beta_1}} - \frac{1}{\sigma^2_{\beta_2}}} = R_{\beta_1\beta_2}\tan 2\phi, \text{ where } \tan\phi = \sigma_{\beta_1}/\sigma_{\beta_2}.$$

The directions were calculated from this formula and Diagram C (p. 389), constructed in such manner that the tangent and normal to one of the broken-line curves at any point are the major and minor axes respectively. The *major* axis is inclined to the axis of β_2 at angles given by Table IX.

A distinction between the directions of the major and minor axes is readily made when we note that a contour of (A) is inscribed in a rectangle of sides $2\sigma_{\beta_1}$ and $2\sigma_{\beta_2}$, and that the major axis of such an ellipse is inclined to the greater side at an angle less than $\pi/4$.

For if $ax^2 + 2hxy + by^2 = 1$ be an ellipse inscribed in a rectangle sides $2q$ and $2p$ having the greater side $2q$ parallel to the axis of x, and centre as origin, then

$$p^2 = \frac{a}{ab - h^2}, \quad q^2 = \frac{b}{ab - h}, \quad \therefore b > a.$$

Again
$$a\cos^2\theta + h\sin 2\theta + b\sin^2\theta = \frac{1}{\gamma^2},$$

$$(b - a)\sin 2\theta + 2h\cos 2\theta = -\frac{2}{\gamma^2}\frac{dr}{d\theta}.$$

For maximum or minimum r

$$\tan 2\theta_1 = \frac{-2h}{b - a},$$

$$2(b - a)\cos 2\theta_1 - 4h\sin 2\theta_1 = -\frac{2}{\gamma_1^3}\frac{d^2r}{d\theta^2} \text{ at a maximum or minimum,}$$

$$\therefore \cos 2\theta_1 \frac{(b-a)^2 + 4h^2}{(b-a)} = -\frac{2}{\gamma_1^2}\frac{d^2r}{d\theta^2}.$$

Hence $\frac{d^2r}{d\theta^2}$ is negative when $\cos 2\theta$ is positive,

$$\therefore \theta \text{ for a maximum is } < \pi/4.$$

If
$$\sin\gamma = \frac{2\sqrt{1 - R^2_{\beta_1\beta_2}}\,\sigma_{\beta_1}\sigma_{\beta_2}}{\sigma^2_{\beta_1} + \sigma^2_{\beta_2}},$$

then
$$\Sigma_1 = \sqrt{\sigma^2_{\beta_1} + \sigma^2_{\beta_2}}\sin\tfrac{1}{2}\gamma,$$

$$\Sigma_2 = \sqrt{\sigma^2_{\beta_1} + \sigma^2_{\beta_2}}\cos\tfrac{1}{2}\gamma.$$

From these formulae Σ_1 and Σ_2 were calculated and the values of $1{\cdot}177\Sigma_1$ and $1{\cdot}177\Sigma_2$ tabulated in VII. and VIII. respectively. The number must of course be multiplied by the factor $\cdot 67449/\sqrt{N}$.

Since the population outside an ellipse of semiaxes $\kappa\Sigma_1$ and $\kappa\Sigma_2$ is $Ne^{-\frac{1}{2}\kappa^2}$, it follows that, if an ellipse of twice the above linear dimensions be drawn, the probability that a point lies outside it is 1/16.

If an ellipse be drawn with semiaxes $\kappa\Sigma_1$ and $\kappa\Sigma_2$, the probability P that a point lies outside the ellipse is $P = e^{-\frac{1}{2}\kappa^2}$. Let

$$\kappa\Sigma_1 = 1{\cdot}177\sqrt{N}\Sigma_1 \times \frac{\cdot 67449}{\sqrt{\frac{N}{q}}}.$$

Then when $q = 10$, $\kappa^2 = 6{\cdot}3006$ and $p = \frac{1}{23}$ approximately,

„ „ $q = 12$, $\kappa^2 = 7{\cdot}5609$ „ $p = \frac{1}{44}$ „

Hence to find the semiaxes of ellipses containing 22/23 or 43/44 of the whole frequency; take the numbers from Tables VII. and VIII. and multiply by the factor from Gibson's Tables (*Biometrika*, Vol. IV. p. 385) corresponding to populations of $N/10$ and $N/12$ respectively.

To facilitate the construction of the ellipse, a series of ellipses has been drawn on Diagram C of different dimensions and varying in ratio of Σ_1/Σ_2 from ·1 to ·3.

In practice the values of the axes should be taken from Tables VII. and VIII. and multiplied by the appropriate factor. Then their ratio gives Σ_1/Σ_2. By means of a piece of tracing paper applied to the corresponding series of ellipses, the required curve can be approximately drawn and transferred to the diagram. It will then be possible to determine whether the frequency really lies with a definite probability within the chosen type, as indicated by the β_1, β_2.

The series of tables has been extended by the inclusion of Table X. which gives the probable error of

$$\kappa_2 = \frac{\beta_1(\beta_2+3)^2}{4(4\beta_2-3\beta_1)(2\beta_2-3\beta_1-6)}.$$

The S.D. was determined thus:

$$\frac{1}{\kappa_2}\delta\kappa_2 = \frac{2}{\beta_2+3}\delta\beta_2 + \frac{1}{\beta_1}\delta\beta_1 - \frac{4\delta\beta_2-3\delta\beta_1}{4\beta_2-3\beta_1} - \frac{2\delta\beta_2-3\delta\beta_1}{(2\beta_2-3\beta_1-6)}$$

$$= \left[\frac{2}{(\beta_2+3)} - \frac{4}{(4\beta_2-3\beta_1)} - \frac{2}{(2\beta_2-3\beta_1-6)}\right]\delta\beta_2$$

$$+ \left[\frac{1}{\beta_1} + \frac{3}{(4\beta_2-3\beta_1)} + \frac{3}{(2\beta_2-3\beta_1-6)}\right]\delta\beta_1,$$

whence

$$\sigma_{\kappa_2} = c^2\{A^2\sigma^2_{\beta_1} + 2ABR_{\beta_1\beta_2}\sigma_{\beta_1}\sigma_{\beta_2} + B^2\sigma^2_{\beta_2}\}^{\frac{1}{2}},$$

where

$$c = \frac{(\beta_2+3)}{2(4\beta_2-3\beta_1)(2\beta_2-3\beta_1-6)},$$

$$A = 8\beta_2(\beta_2-3) - 9\beta_1^2,$$

$$B = 18\beta_1\frac{4+\beta_1(\beta_1+5)-\beta_2(\beta_1+4)}{(\beta_2+3)}.$$

Erratum. In the printing of my former paper a slip of sign escaped my notice. On p. 129, *Biometrika*, Vol. VII., l. 7, for $\alpha = (2\beta_2 - 3\beta_1 - 6)/(\beta_2 - 3)$ read $\alpha = (2\beta_2 - 3\beta_1 - 6)/(\beta_2 + 3)$, which was, of course, the value used in the calculation of the tables and diagrams.

DIAGRAM C.

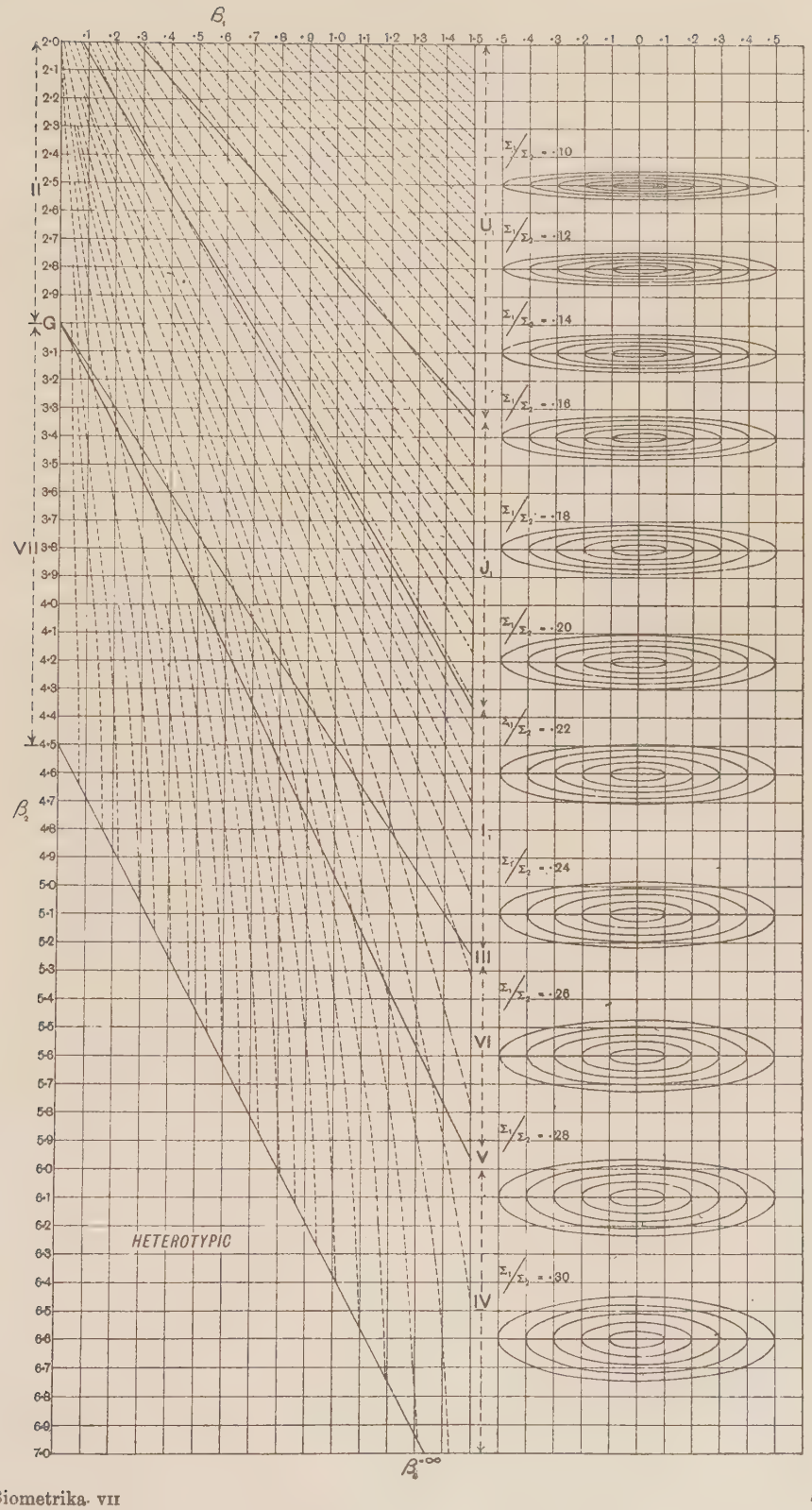

β_1
β_2
$\beta_2 \cdot \infty$
$\Sigma_1/\Sigma_2 = .10$
$\Sigma_1/\Sigma_2 = .12$
$\Sigma_1/\Sigma_2 = .14$
$\Sigma_1/\Sigma_2 = .16$
$\Sigma_1/\Sigma_2 = .18$
$\Sigma_1/\Sigma_2 = .20$
$\Sigma_1/\Sigma_2 = .22$
$\Sigma_1/\Sigma_2 = .24$
$\Sigma_1/\Sigma_2 = .26$
$\Sigma_1/\Sigma_2 = .28$
$\Sigma_1/\Sigma_2 = .30$
HETEROTYPIC
II
G
VII
U
J
I
III
VI
V
IV

TABLE VII.

Values of $1{\cdot}77\sqrt{N}\Sigma_1$ *for given values of* β_1, β_2 (*Semi-Minor Axis of Probability Ellipse*).

β_2 \ β_1	0	·05	·1	·15	·2	·25	·3	·35	·4	·45	·5	·55	·6	·65	·7	·75
2·0	0·0	0·5	0·7	0·8	0·8	0·8	0·8	0·8	0·7	0·7	0·7	0·6	0·6	0·5	0·5	0·4
2·1	0·0	0·5	0·7	0·8	0·9	0·9	0·8	0·8	0·8	0·8	0·7	0·7	0·7	0·6	0·6	0·5
2·2	0·0	0·5	0·8	0·9	0·9	0·9	0·9	0·9	0·9	0·8	0·8	0·8	0·7	0·7	0·7	0·6
2·3	0·0	0·5	0·8	0·9	1·0	1·0	1·0	1·0	1·0	0·9	0·9	0·9	0·8	0·8	0·8	0·7
2·4	0·0	0·6	0·9	1·0	1·0	1·0	1·0	1·0	1·0	1·0	1·0	0·9	0·9	0·9	0·9	0·8
2·5	0·0	0·6	0·9	1·0	1·1	1·1	1·1	1·1	1·1	1·1	1·0	1·0	1·0	1·0	1·0	0·9
2·6	0·0	0·6	1·0	1·1	1·1	1·2	1·2	1·2	1·1	1·1	1·1	1·1	1·1	1·0	1·0	1·0
2·7	0·0	0·7	1·0	1·2	1·2	1·3	1·3	1·3	1·2	1·2	1·2	1·2	1·1	1·1	1·1	1·1
2·8	0·0	0·7	1·1	1·3	1·3	1·3	1·3	1·3	1·3	1·3	1·3	1·3	1·2	1·2	1·2	1·2
2·9	0·0	0·8	1·2	1·3	1·3	1·4	1·4	1·4	1·4	1·4	1·4	1·4	1·3	1·3	1·3	1·3
3·0	0·0	0·8	1·2	1·4	1·4	1·5	1·5	1·5	1·5	1·5	1·5	1·5	1·4	1·4	1·4	1·4
3·1	0·0	0·9	1·3	1·5	1·5	1·6	1·6	1·6	1·6	1·6	1·6	1·6	1·6	1·5	1·5	1·5
3·2	0·0	1·0	1·4	1·6	1·6	1·7	1·7	1·7	1·7	1·7	1·7	1·7	1·7	1·6	1·6	1·6
3·3	·00	1·1	1·6	1·8	1·8	1·9	1·9	1·9	1·8	1·8	1·8	1·8	1·8	1·7	1·7	1·7
3·4	·00	1·2	1·7	1·9	1·9	2·0	2·0	2·0	2·0	2·0	1·9	1·9	1·9	1·9	1·9	1·8
3·5	·00	1·4	1·8	2·0	2·1	2·1	2·1	2·1	2·1	2·1	2·1	2·1	2·1	2·0	2·0	1·9
3·6	·00	1·5	2·0	2·1	2·2	2·3	2·3	2·3	2·3	2·3	2·3	2·2	2·2	2·1	2·1	2·1
3·7	·00	1·6	2·1	2·3	2·4	2·5	2·5	2·5	2·5	2·5	2·4	2·4	2·4	2·3	2·3	2·3
3·8	·00	1·8	2·3	2·5	2·6	2·7	2·7	2·7	2·7	2·7	2·6	2·6	2·6	2·5	2·5	2·4
3·9	·00	2·0	2·5	2·7	2·9	3·0	3·0	3·0	3·0	2·9	2·9	2·8	2·8	2·7	2·7	2·6
4·0	·00	2·2	2·8	3·0	3·2	3·3	3·3	3·3	3·3	3·2	3·1	3·1	3·0	2·9	2·9	2·8
4·1	—	—	3·0	3·3	3·5	3·6	3·6	3·6	3·6	3·5	3·4	3·4	3·3	3·2	3·2	3·1
4·2	—	—	3·3	3·6	3·8	3·9	4·0	4·0	3·9	3·8	3·8	3·7	3·6	3·5	3·5	3·4
4·3	—	—	3·6	4·0	4·2	4·3	4·4	4·4	4·3	4·2	4·1	4·0	3·9	3·8	3·8	3·7
4·4	—	—	4·0	4·4	4·7	4·8	4·8	4·8	4·7	4·6	4·5	4·4	4·3	4·2	4·1	4·0
4·5	—	—	4·4	4·9	5·2	5·3	5·3	5·3	5·2	5·1	5·0	4·9	4·7	4·6	4·5	4·4
4·6	—	—	—	—	—	—	5·9	5·9	5·8	5·7	5·6	5·4	5·2	5·1	5·0	4·8
4·7	—	—	—	—	—	—	6·7	6·7	6·6	6·4	6·2	6·0	5·8	5·7	5·6	5·3
4·8	—	—	—	—	—	—	7·6	7·6	7·4	7·2	6·9	6·6	6·4	6·3	6·2	5·9
4·9	—	—	—	—	—	—	8·5	8·5	8·4	8·1	7·8	7·5	7·2	7·0	6·8	6·5
5·0	—	—	—	—	—	—	11	10	9·7	9·3	8·9	8·5	8·1	7·7	7·4	7·1
5·1	—	—	—	—	—	—	—	—	—	—	—	—	9·3	8·6	8·1	7·7
5·2	—	—	—	—	—	—	—	—	—	—	—	—	11	9·5	8·9	8·6
5·3	—	—	—	—	—	—	—	—	—	—	—	—	12	11	10	9·6
5·4	—	—	—	—	—	—	—	—	—	—	—	—	14	13	12	11
5·5	—	—	—	—	—	—	—	—	—	—	—	—	16	15	14	13
5·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
7·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

TABLE VII.—(*continued*).

Values of $1{\cdot}77\sqrt{N}\,\Sigma_1$ *for given values of* β_1, β_2 (*Semi-Minor Axis of Probability Ellipse*).

β_1															
·8	·85	·9	·95	1·0	1·05	1·1	1·15	1·2	1·25	1·3	1·35	1·4	1·45	1·5	
0·4	0·3	0·3	0·2	0·1	0·1	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	2·0
0·5	0·5	0·4	0·3	0·2	0·2	0·2	0·1	0·1	0·1	0·0	0·0	0·0	0·0	0·0	2·1
0·6	0·6	0·5	0·5	0·4	0·4	0·3	0·3	0·3	0·2	0·1	0·0	0·0	0·0	0·0	2·2
0·7	0·7	0·6	0·6	0·5	0·5	0·4	0·4	0·4	0·3	0·2	0·1	0·1	0·0	0·0	2·3
0·8	0·8	0·7	0·7	0·6	0·6	0·5	0·5	0·4	0·4	0·3	0·2	0·2	0·1	0·0	2·4
0·9	0·8	0·8	0·7	0·7	0·7	0·6	0·6	0·5	0·5	0·4	0·3	0·3	0·2	0·1	2·5
1·0	0·9	0·9	0·8	0·8	0·8	0·7	0·7	0·7	0·6	0·6	0·5	0·4	0·3	0·2	2·6
1·1	1·0	1·0	0·9	0·9	0·9	0·8	0·8	0·8	0·7	0·7	0·6	0·5	0·4	0·3	2·7
1·1	1·1	1·1	1·0	1·0	1·0	0·9	0·9	0·9	0·8	0·8	0·7	0·6	0·5	0·5	2·8
1·2	1·2	1·2	1·1	1·1	1·0	1·0	1·0	1·0	1·0	0·9	0·8	0·7	0·7	0·7	2·9
1·3	1·3	1·3	1·2	1·2	1·2	1·1	1·1	1·1	1·0	1·0	0·9	0·9	0·8	0·8	3·0
1·4	1·4	1·4	1·3	1·3	1·3	1·2	1·2	1·2	1·1	1·1	1·0	1·0	0·9	0·9	3·1
1·5	1·5	1·5	1·4	1·4	1·4	1·3	1·3	1·2	1·2	1·2	1·1	1·1	1·0	1·0	3·2
1·7	1·6	1·6	1·5	1·4	1·4	1·3	1·3	1·3	1·2	1·2	1·2	1·1	1·1	1·0	3·3
1·8	1·7	1·7	1·6	1·6	1·6	1·5	1·5	1·4	1·4	1·3	1·3	1·2	1·2	1·1	3·4
1·9	1·9	1·8	1·8	1·7	1·7	1·6	1·6	1·5	1·5	1·4	1·4	1·3	1·3	1·2	3·5
2·0	2·0	2·0	1·9	1·8	1·8	1·7	1·7	1·6	1·6	1·5	1·5	1·4	1·4	1·4	3·6
2·2	2·2	2·1	2·1	2·0	2·0	1·9	1·8	1·7	1·7	1·6	1·6	1·6	1·5	1·5	3·7
2·4	2·3	2·2	2·2	2·1	2·1	2·0	2·0	1·9	1·9	1·8	1·7	1·7	1·6	1·6	3·8
2·6	2·5	2·4	2·4	2·3	2·3	2·2	2·1	2·0	2·0	1·9	1·9	1·8	1·7	1·7	3·9
2·8	2·7	2·6	2·6	2·5	2·5	2·4	2·3	2·2	2·2	2·1	2·1	2·0	2·0	1·9	4·0
3·0	2·9	2·8	2·8	2·7	2·7	2·6	2·5	2·4	2·4	2·3	2·3	2·2	2·2	2·1	4·1
3·2	3·2	3·1	3·0	2·9	2·9	2·8	2·7	2·6	2·6	2·5	2·4	2·3	2·3	2·2	4·2
3·5	3·5	3·4	3·3	3·2	3·1	3·0	2·9	2·8	2·8	2·7	2·6	2·5	2·4	2·4	4·3
3·9	3·8	3·7	3·6	3·5	3·4	3·3	3·1	3·0	3·0	2·9	2·8	2·7	2·7	2·6	4·4
4·2	4·1	4·0	3·9	3·7	3·6	3·5	3·4	3·3	3·2	3·1	3·1	3·0	3·0	2·9	4·5
4·6	4·5	4·4	4·2	4·1	3·9	3·8	3·7	3·5	3·4	3·3	3·3	3·2	3·2	3·1	4·6
5·1	4·9	4·8	4·6	4·4	4·3	4·2	4·0	3·8	3·7	3·6	3·5	3·4	3·4	3·3	4·7
5·6	5·4	5·2	5·0	4·8	4·7	4·6	4·4	4·2	4·0	3·9	3·8	3·7	3·6	3·5	4·8
6·2	6·0	5·7	5·5	5·3	5·1	5·0	4·8	4·6	4·4	4·2	4·1	4·0	3·9	3·8	4·9
6·8	6·6	6·3	6·1	5·8	5·6	5·4	5·2	5·0	4·8	4·6	4·4	4·3	4·2	4·1	5·0
7·4	7·2	7·0	6·7	6·4	6·2	5·9	5·7	5·4	5·1	4·9	4·7	4·6	4·4	4·3	5·1
8·3	8·0	7·7	7·4	7·1	6·8	6·5	6·2	5·8	5·5	5·2	5·0	4·9	4·7	4·5	5·2
9·2	8·9	8·6	8·2	7·8	7·5	7·1	6·7	6·3	5·9	5·6	5·4	5·2	5·0	4·7	5·3
10	10	9·6	9·1	8·6	8·2	7·8	7·3	6·9	6·5	6·1	5·8	5·6	5·3	5·0	5·4
12	11	11	10	9·5	9·0	8·5	8·0	7·5	7·1	6·7	6·4	6·1	5·8	5·5	5·5
—	—	12	11	10	10	9·5	8·9	8·4	7·9	7·4	7·0	6·6	6·3	6·0	5·6
—	—	14	13	12	11	10	10	9·4	8·8	8·3	7·8	7·4	7·0	6·7	5·7
—	—	16	14	13	12	12	11	10	10	9·4	8·8	8·3	7·8	7·4	5·8
—	—	18	16	15	14	14	13	12	11	10	10	9·5	8·9	8·4	5·9
—	—	22	20	18	16	15	14	13	12	12	11	11	10	9·5	6·0
—	—	—	—	—	—	18	16	15	14	13	13	12	12	11	6·1
—	—	—	—	—	—	21	19	17	16	15	14	13	13	12	6·2
—	—	—	—	—	—	24	21	19	18	17	16	15	14	13	6·3
—	—	—	—	—	—	27	24	22	20	19	18	17	16	15	6·4
—	—	—	—	—	—	32	28	25	23	21	20	19	18	16	6·5
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6·6
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6·7
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6·8
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6·9
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	7·0

β_2

TABLE VIII.

Values of $1{\cdot}77\sqrt{N}\,\Sigma_2$ *for values of* β_1, β_2 (*Semi-Major Axis of Probability Ellipse*).

β_2 \ β_1	0	·05	·1	·15	·2	·25	·3	·35	·4	·45	·5	·55	·6	·65	·7	·75
2·0	1·6	1·9	2·2	2·5	2·8	3·1	3·4	3·7	4·1	4·4	4·7	5·0	5·4	5·7	6·1	6·4
2·1	1·7	2·1	2·4	2·7	2·9	3·2	3·5	3·8	4·1	4·4	4·6	4·9	5·3	5·6	6·0	6·3
2·2	1·9	2·3	2·6	2·9	3·1	3·4	3·7	3·9	4·1	4·4	4·6	4·9	5·2	5·5	5·9	6·2
2·3	2·1	2·5	2·8	3·1	3·3	3·5	3·8	4·0	4·2	4·4	4·6	4·9	5·2	5·5	5·8	6·1
2·4	2·4	2·8	3·1	3·3	3·5	3·7	3·9	4·1	4·3	4·5	4·7	4·9	5·1	5·4	5·7	6·0
2·5	2·8	3·3	3·5	3·7	3·8	4·0	4·1	4·3	4·5	4·7	4·8	5·0	5·2	5·4	5·7	6·0
2·6	3·3	3·8	4·0	4·2	4·3	4·5	4·6	4·7	4·8	4·9	5·0	5·1	5·3	5·5	5·7	6·0
2·7	3·8	4·4	4·6	4·7	4·9	5·0	5·1	5·2	5·3	5·4	5·4	5·4	5·5	5·6	5·8	6·1
2·8	4·4	5·0	5·3	5·4	5·6	5·7	5·8	5·9	5·9	5·9	5·8	5·8	5·8	5·9	6·0	6·2
2·9	5·1	5·7	6·0	6·2	6·3	6·4	6·5	6·6	6·5	6·5	6·4	6·3	6·3	6·3	6·4	6·5
3·0	5·8	6·5	6·9	7·1	7·2	7·3	7·3	7·3	7·2	7·1	7·0	6·9	6·8	6·8	6·8	6·8
3·1	6·7	7·5	8·0	8·3	8·4	8·4	8·4	8·3	8·2	8·0	7·9	7·8	7·7	7·6	7·5	7·4
3·2	7·8	8·7	9·2	9·6	9·8	9·8	9·7	9·5	9·4	9·2	9·0	8·8	8·6	8·4	8·3	8·1
3·3	9·2	10	11	11	11	11	11	11	11	11	10	10	9·6	9·4	9·2	8·9
3·4	11	11	12	12	13	13	13	12	12	12	11	11	11	11	10	10
3·5	13	13	14	14	15	15	14	14	14	14	13	12	12	12	11	11
3·6	15	16	17	17	17	17	16	16	16	16	15	14	14	14	13	13
3·7	18	19	20	20	20	20	19	19	18	18	17	17	16	16	15	14
3·8	21	23	24	24	24	23	22	22	21	21	20	19	18	18	17	16
3·9	25	27	28	28	28	27	26	25	24	24	23	22	21	20	19	18
4·0	29	32	34	33	32	31	30	29	28	27	26	24	23	22	21	20
4·1	—	—	43	41	39	37	36	34	32	31	30	28	26	25	24	23
4·2	—	—	55	51	48	45	42	40	38	36	34	32	30	29	27	26
4·3	—	—	69	62	57	53	50	48	45	43	40	37	35	33	31	29
4·4	—	—	87	76	69	64	60	56	53	50	46	43	40	38	36	34
4·5	—	—	113	98	86	78	71	66	62	57	53	49	46	43	41	39
4·6	—	—	—	—	—	—	90	79	73	67	61	57	53	50	48	45
4·7	—	—	—	—	—	—	110	99	90	80	72	67	62	58	55	52
4·8	—	—	—	—	—	—	140	124	110	97	86	79	73	68	64	60
4·9	—	—	—	—	—	—	200	160	135	119	105	95	86	79	74	69
5·0	—	—	—	—	—	—	384	234	182	153	130	115	102	92	85	79
5·1	—	—	—	—	—	—	—	—	—	—	—	—	124	110	101	93
5·2	—	—	—	—	—	—	—	—	—	—	—	—	148	134	120	109
5·3	—	—	—	—	—	—	—	—	—	—	—	—	188	166	146	129
5·4	—	—	—	—	—	—	—	—	—	—	—	—	245	203	177	154
5·5	—	—	—	—	—	—	—	—	—	—	—	—	405	297	230	194
5·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
7·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

TABLE VIII.—(*continued*).

Values of $1{\cdot}77\sqrt{N}\,\Sigma_2$ *for values of* β_1, β_2 (*Semi-Major Axis of Probability Ellipse*).

β_1															
·8	·85	·9	·95	1·0	1·05	1·1	1·15	1·2	1·25	1·3	1·35	1·4	1·45	1·5	
6·8	7·2	7·6	7·9	8·3	8·7	9·1	9·5	9·9	10	11	11	11	12	12	2·0
6·7	7·0	7·4	7·8	8·1	8·5	8·9	9·2	9·6	10	11	11	11	11	12	2·1
6·5	6·8	7·2	7·6	8·0	8·3	8·6	9·0	9·4	9·8	10	10	11	11	12	2·2
6·4	6·7	7·1	7·4	7·8	8·1	8·5	8·8	9·2	9·6	9·9	10	11	11	12	2·3
6·3	6·6	6·9	7·3	7·6	8·0	8·3	8·7	9·0	9·4	9·8	10	10	11	11	2·4
6·2	6·5	6·8	7·1	7·5	7·8	8·2	8·5	8·9	9·2	9·6	10	10	11	11	2·5
6·2	6·5	6·8	7·1	7·4	7·7	8·0	8·3	8·7	9·0	9·4	9·9	10	11	11	2·6
6·3	6·5	6·8	7·0	7·3	7·6	7·9	8·2	8·5	8·8	9·2	9·7	10	10	11	2·7
6·4	6·6	6·8	7·0	7·2	7·5	7·7	8·0	8·3	8·7	9·1	9·5	9·8	10	10	2·8
6·6	6·7	6·8	7·0	7·2	7·4	7·6	7·9	8·2	8·6	8·9	9·3	9·6	10	10	2·9
6·9	7·0	7·1	7·2	7·3	7·5	7·7	7·9	8·2	8·5	8·8	9·1	9·4	9·7	10	3·0
7·3	7·3	7·3	7·4	7·5	7·6	7·8	8·0	8·2	8·5	8·7	9·0	9·3	9·6	9·9	3·1
7·9	7·9	7·8	7·8	7·8	7·9	8·1	8·2	8·3	8·5	8·7	8·9	9·2	9·5	9·8	3·2
8·7	8·5	8·4	8·3	8·3	8·3	8·3	8·4	8·5	8·7	8·9	9·1	9·3	9·5	9·7	3·3
9·5	9·3	9·1	9·0	8·8	8·8	8·8	8·8	8·9	9·0	9·1	9·2	9·4	9·5	9·7	3·4
11	10	10	9·8	9·6	9·5	9·4	9·3	9·3	9·3	9·3	9·4	9·5	9·6	9·8	3·5
12	11	11	11	10	10	10	10	9·9	9·9	9·8	9·7	9·7	9·8	9·9	3·6
13	12	12	12	11	11	11	11	11	10	10	10	10	10	10	3·7
15	14	14	13	13	13	12	12	12	11	11	10	10	10	11	3·8
17	16	16	15	14	14	14	13	13	12	12	11	11	11	11	3·9
19	18	18	17	16	16	15	14	14	13	13	12	12	12	12	4·0
22	21	20	19	18	17	17	16	15	15	14	14	13	13	13	4·1
24	23	22	21	20	19	18	18	17	16	15	15	14	14	14	4·2
28	26	25	23	22	21	20	20	19	18	17	16	15	15	15	4·3
32	30	29	27	25	24	23	22	21	20	19	18	17	17	17	4·4
37	35	33	31	29	27	26	25	24	23	22	21	20	19	18	4·5
43	40	37	35	33	31	29	28	27	25	24	23	22	21	20	4·6
49	45	42	39	37	35	33	32	30	28	27	26	24	23	22	4·7
56	52	48	45	42	40	38	36	34	32	31	29	27	26	25	4·8
64	59	55	51	48	46	42	40	38	36	35	33	31	29	28	4·9
73	68	63	59	55	52	49	46	43	41	39	37	35	33	32	5·0
85	78	72	67	63	59	55	52	49	46	43	41	39	37	36	5·1
99	90	82	77	72	67	63	58	55	52	49	46	43	41	40	5·2
114	104	95	88	82	76	71	66	62	58	55	51	48	46	44	5·3
136	123	112	102	95	87	80	75	70	65	61	57	54	51	49	5·4
167	147	132	119	108	100	92	85	79	74	69	64	60	57	54	5·5
—	—	160	141	126	115	105	96	89	83	78	73	68	64	60	5·6
—	—	206	169	148	132	120	110	102	95	88	82	76	72	67	5·7
—	—	258	206	175	150	136	126	116	108	100	93	87	81	75	5·8
—	—	318	255	215	190	168	150	136	125	115	107	99	92	85	5·9
—	—	446	332	273	228	200	178	161	147	134	123	113	104	98	6·0
—	—	—	—	—	—	264	215	190	171	157	144	130	120	112	6·1
—	—	—	—	—	—	345	268	230	207	184	167	150	138	127	6·2
—	—	—	—	—	—	480	364	294	250	215	194	174	159	144	6·3
—	—	—	—	—	—	680	477	370	299	252	224	201	181	165	6·4
—	—	—	—	—	—	1047	680	456	368	312	268	237	212	191	6·5
—	—	—	—	—	—	—	—	—	—	—	—	280	248	223	6·6
—	—	—	—	—	—	—	—	—	—	—	—	338	297	266	6·7
—	—	—	—	—	—	—	—	—	—	—	—	412	362	320	6·8
—	—	—	—	—	—	—	—	—	—	—	—	525	446	390	6·9
—	—	—	—	—	—	—	—	—	—	—	—	809	584	491	7·0

β_2

TABLE IX.

Angle between Major-Axis and Axis of β_2 (Probability Ellipse) measured in degrees.

β_2 \ β_1	0	·05	·1	·15	·2	·25	·3	·35	·4	·45	·5	·55	·6	·65	·7	·75
2·0	0	12	23	28	31	33	35	36	37	38	39	40	41	41	42	42
2·1	0	11	21	25	28	30	32	34	35	37	38	39	40	40	41	41
2·2	0	10	19	23	26	28	30	32	33	35	36	38	39	39	40	40
2·3	0	10	18	22	25	27	28	30	32	34	35	37	38	38	39	39
2·4	0	9	17	20	23	25	26	29	31	33	34	35	36	37	38	38
2·5	0	8	15	18	21	23	25	27	29	31	33	34	35	36	37	37
2·6	0	7	14	17	20	22	24	26	28	30	31	33	34	35	35	36
2·7	0	7	13	16	19	21	23	25	26	28	29	31	32	33	34	35
2·8	0	6	12	15	17	19	21	23	25	27	28	30	31	32	33	33
2·9	0	6	11	14	16	18	20	22	23	25	26	28	29	30	31	32
3·0	0	5	10	13	15	17	19	21	22	24	25	27	28	29	30	31
3·1	0	5	9	12	14	16	18	20	21	23	24	26	27	28	29	30
3·2	0	5	9	12	14	16	17	19	20	21	22	24	25	26	27	28
3·3	0	4	8	11	13	15	16	18	19	20	21	22	24	25	26	27
3·4	0	4	8	10	12	14	15	17	18	19	20	21	22	23	24	25
3·5	0	3	7	9	11	13	14	15	17	18	19	20	21	22	23	24
3·6	0	3	6	8	10	12	13	14	15	16	18	19	20	21	22	23
3·7	0	3	5	7	9	11	12	13	14	15	16	17	18	19	20	21
3·8	0	3	5	7	9	10	11	12	13	14	15	16	17	18	19	20
3·9	0	2	4	6	8	9	10	11	12	13	14	15	16	17	18	19
4·0	0	2	4	6	7	8	9	10	11	12	13	14	15	16	17	18
4·1	—	—	3	5	6	7	8	9	10	11	12	13	14	15	16	17
4·2	—	—	3	4	5	6	7	8	9	10	11	12	13	14	15	16
4·3	—	—	2	4	5	5	6	7	9	10	10	11	12	13	14	15
4·4	—	—	2	3	4	5	6	7	8	9	9	10	11	12	13	14
4·5	—	—	1	2	3	4	5	6	7	8	8	9	10	11	12	13
4·6	—	—	—	—	—	—	4	5	6	7	7	8	9	10	11	12
4·7	—	—	—	—	—	—	3	4	5	6	6	7	8	9	10	11
4·8	—	—	—	—	—	—	3	4	4	5	5	6	7	8	9	10
4·9	—	—	—	—	—	—	2	3	3	4	5	6	6	7	8	9
5·0	—	—	—	—	—	—	1	2	2	3	4	5	6	6	7	8
5·1	—	—	—	—	—	—	—	—	—	—	—	—	5	5	6	7
5·2	—	—	—	—	—	—	—	—	—	—	—	—	4	5	6	7
5·3	—	—	—	—	—	—	—	—	—	—	—	—	3	4	5	5
5·4	—	—	—	—	—	—	—	—	—	—	—	—	2	3	4	5
5·5	—	—	—	—	—	—	—	—	—	—	—	—	1	2	3	4
5·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
7·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

TABLE IX.—(*continued*).

Angle between Major-Axis and Axis of β_2 (Probability Ellipse) measured in degrees.

β_1															
·8	·85	·9	·95	1·0	1·05	1·1	1·15	1·2	1·25	1·3	1·35	1·4	1·45	1·5	
43	43	44	44	45	45	46	46	46	46	47	47	48	48	49	2·0
42	42	43	44	44	44	45	45	45	46	46	47	47	48	48	2·1
41	41	42	42	43	43	44	44	45	45	46	46	46	47	47	2·2
40	40	41	41	42	42	43	43	44	44	45	45	46	46	47	2·3
39	39	40	40	41	41	42	42	43	43	44	44	45	45	46	2·4
38	38	39	39	40	40	41	41	42	42	43	43	44	44	45	2·5
37	37	38	39	39	39	40	40	41	41	42	42	43	43	44	2·6
36	36	37	38	38	39	39	40	41	41	42	42	43	43	44	2·7
34	35	36	36	37	38	38	39	40	40	41	41	42	42	43	2·8
33	34	35	35	36	37	38	38	39	39	40	40	41	41	42	2·9
32	33	34	34	35	36	37	37	38	38	39	39	40	40	41	3·0
30	31	32	33	34	35	35	36	37	38	38	39	39	40	40	3·1
29	30	31	32	33	34	34	35	36	37	38	38	39	39	39	3·2
28	29	30	31	32	32	33	34	35	36	37	37	38	38	38	3·3
26	27	28	29	30	31	32	33	34	35	36	36	37	38	38	3·4
25	26	27	28	29	30	31	32	33	34	35	35	36	36	37	3·5
24	25	26	27	28	29	30	31	32	33	34	34	35	35	36	3·6
22	23	24	25	26	27	28	29	31	32	33	33	34	34	35	3·7
21	22	23	24	25	26	27	28	29	30	31	32	33	33	34	3·8
20	21	22	23	24	25	26	27	28	29	30	31	32	32	33	3·9
19	20	21	22	23	24	25	26	27	28	29	30	31	31	32	4·0
18	19	20	21	22	23	24	25	26	27	28	29	30	30	31	4·1
17	18	19	19	20	21	22	23	25	26	27	28	29	30	30	4·2
16	17	18	19	19	20	21	22	23	24	25	26	27	28	29	4·3
15	16	17	18	18	19	20	21	22	23	24	25	26	27	28	4·4
14	15	16	16	17	18	19	20	21	22	23	24	25	25	26	4·5
13	14	15	15	16	17	18	19	20	21	22	23	24	25	25	4·6
12	13	14	14	15	16	17	18	19	20	21	22	23	24	24	4·7
11	12	13	14	15	15	16	17	18	19	20	21	22	23	23	4·8
10	11	12	13	14	14	15	16	17	18	19	20	21	21	22	4·9
9	10	11	12	13	13	14	15	16	17	18	19	20	20	21	5·0
8	9	10	11	12	13	13	15	16	16	17	18	19	20	20	5·1
7	8	9	10	11	12	13	14	15	15	16	17	18	19	19	5·2
6	7	8	9	10	11	12	13	14	14	15	16	17	18	18	5·3
6	7	8	8	9	10	11	12	13	13	14	15	16	17	17	5·4
5	6	7	7	8	9	10	11	12	12	13	14	15	16	17	5·5
—	—	6	7	7	8	9	10	11	12	13	13	14	15	16	5·6
—	—	5	6	6	7	8	9	10	11	12	12	13	14	15	5·7
—	—	4	5	5	6	7	8	9	10	11	11	12	13	14	5·8
—	—	3	4	5	5	6	7	8	9	10	10	11	12	13	5·9
—	—	2	3	4	4	5	6	7	8	9	9	10	11	12	6·0
—	—	—	—	—	—	—	—	6	7	8	9	9	10	11	6·1
—	—	—	—	—	—	—	—	5	6	7	8	8	9	10	6·2
—	—	—	—	—	—	—	—	4	5	6	6	7	8	9	6·3
—	—	—	—	—	—	—	—	3	4	5	6	6	7	8	6·4
—	—	—	—	—	—	—	—	2	3	4	5	6	6	7	6·5
—	—	—	—	—	—	—	—	—	—	—	—	5	6	7	6·6
—	—	—	—	—	—	—	—	—	—	—	—	4	5	6	6·7
—	—	—	—	—	—	—	—	—	—	—	—	3	4	5	6·8
—	—	—	—	—	—	—	—	—	—	—	—	2	3	4	6·9
—	—	—	—	—	—	—	—	—	—	—	—	1	3	4	7·0

β_2

TABLE X.

Probable Error of Criterion κ_2. *Values of* $\sqrt{N}\Sigma_{\kappa_2}$ *for values of* β_1, β_2.

β_2 \ β_1	·00	·05	·10	·15	·20	·25	·30	·35	·40	·45	·50	·55	·60	·65	·70
2·0	·000	·242	·332	·399	·454	·498	·545	·582	·631	·671	·716	·758	·806	·854	·899
2·1	·000	·271	·367	·430	·483	·521	·557	·600	·639	·678	·717	·760	·800	·843	·890
2·2	·000	·310	·415	·480	·527	·565	·597	·626	·656	·691	·728	·767	·809	·845	·890
2·3	·000	·355	·477	·550	·596	·635	·660	·678	·700	·725	·753	·790	·826	·858	·895
2·4	·000	·417	·560	·642	·691	·722	·748	·759	·770	·787	·806	·830	·857	·884	·914
2·5	·000	·500	·697	·771	·816	·841	·855	·860	·867	·873	·881	·892	·909	·928	·947
2·6	·000	·660	·840	·946	1·01	1·03	1·04	1·02	1·01	1·00	1·00	1·00	1·00	1·00	1·00
2·7	·000	1·04	1·30	1·35	1·34	1·33	1·30	1·27	1·24	1·19	1·15	1·12	1·10	1·09	1·08
2·8	·000	1·83	1·97	1·98	1·93	1·84	1·74	1·64	1·55	1·47	1·40	1·33	1·27	1·23	1·20
2·9	·000	3·51	3·71	3·42	3·00	2·66	2·42	2·22	2·05	1·89	1·73	1·61	1·51	1·45	1·39
3·0	—	18·8	9·89	6·94	5·30	4·30	3·62	3·10	2·73	2·43	2·18	2·00	1·84	1·71	1·61
3·1	·000	—	62·0	20·5	8·47	6·36	5·22	4·42	3·78	3·34	2·96	2·66	2·41	2·20	2·01
3·2	·000	7·82	91·7	—	49·7	20·2	12·2	8·48	6·44	5·09	4·28	3·71	3·21	2·86	2·58
3·3	·000	2·99	11·7	70·2	—	142	32·4	15·4	10·8	8·56	6·88	5·62	4·66	3·98	3·46
3·4	·000	1·82	4·80	13·8	55·5	—	344	182	28·9	16·8	11·8	8·69	6·88	5·60	4·76
3·5	·000	1·43	2·89	6·43	15·8	50·6	380	—	127	46·5	25·1	16·1	11·5	8·55	6·91
3·6	·000	1·17	2·18	4·08	8·00	17·2	46·5	215	—	277	76·4	35·4	22·4	15·8	11·5
3·7	·000	1·04	1·79	3·08	5·18	9·36	24·6	44·3	155	—	767	133	54·6	28·5	19·2
3·8	·000	·979	1·54	2·54	4·09	6·33	14·3	26·2	44·0	126	855	—	230	80·0	40·8
3·9	·000	·920	1·41	2·20	3·32	4·91	8·84	13·1	20·5	38·2	105	448	—	—	125
4·0	·000	·869	1·35	2·00	2·83	4·08	5·98	9·08	13·7	22·3	41·4	98·0	296	—	—
4·1	—	—	1·30	1·94	2·60	3·61	5·08	7·25	10·4	15·8	24·4	40·5	78·7	216	—
4·2	—	—	1·33	1·94	2·58	3·43	4·54	6·09	8·49	11·8	16·8	25·3	39·4	67·2	169
4·3	—	—	1·44	2·01	2·63	3·37	4·20	5·41	7·19	9·52	12·8	17·8	26·2	45·2	72·2
4·4	—	—	1·58	2·15	2·74	3·39	4·20	5·18	6·58	8·56	10·9	14·6	20·2	29·0	40·4
4·5	—	—	1·81	2·32	2·94	3·59	4·36	5·29	6·45	7·76	9·85	12·4	16·1	21·1	29·9
4·6	—	—	—	—	—	—	4·90	5·63	6·60	7·84	9·20	11·1	13·6	17·2	24·2
4·7	—	—	—	—	—	—	5·87	6·46	7·11	7·97	8·96	10·2	12·2	15·3	20·0
4·8	—	—	—	—	—	—	7·43	7·61	7·94	8·41	9·16	10·1	12·0	14·3	17·5
4·9	—	—	—	—	—	—	10·1	9·45	9·08	9·36	9·80	10·6	12·0	13·7	15·9
5·0	—	—	—	—	—	—	15·1	11·3	10·4	10·4	10·8	11·5	12·4	13·6	15·4
5·1	—	—	—	—	—	—	—	—	—	—	—	—	13·5	14·0	15·3
5·2	—	—	—	—	—	—	—	—	—	—	—	—	15·3	15·5	16·0
5·3	—	—	—	—	—	—	—	—	—	—	—	—	18·3	17·8	17·7
5·4	—	—	—	—	—	—	—	—	—	—	—	—	22·8	20·6	20·0
5·5	—	—	—	—	—	—	—	—	—	—	—	—	30·6	26·0	23·3
5·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
6·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
7·0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

TABLE X.—(*continued*).

Probable Error of Criterion κ_2. *Values of* $\sqrt{N}\Sigma_{\kappa_2}$ *for values of* β_1, β_2.

β_1

·75	·80	·85	·90	·95	1·00	1·05	1·10	1·15	1·20	1·25	1·30	1·35	1·40	1·45	1·50	
·949	1·00	1·06	1·12	1·18	1·25	1·31	1·39	1·46	1·56	1·64	1·75	1·86	1·97	2·11	2·25	*2·0*
·937	·992	1·05	1·10	1·16	1·22	1·28	1·35	1·42	1·49	1·58	1·67	1·77	1·88	2·00	2·12	*2·1*
·936	·987	1·04	1·09	1·14	1·19	1·25	1·31	1·37	1·43	1·51	1·59	1·69	1·79	1·89	1·99	*2·2*
·939	·982	1·04	1·08	1·12	1·17	1·22	1·27	1·32	1·38	1·45	1·53	1·62	1·70	1·79	1·87	*2·3*
·950	·990	1·03	1·07	1·11	1·15	1·19	1·24	1·29	1·34	1·40	1·47	1·55	1·62	1·69	1·77	*2·4*
·972	·998	1·03	1·07	1·10	1·14	1·18	1·22	1·27	1·32	1·37	1·43	1·49	1·55	1·61	1·68	*2·5*
1·01	1·03	1·06	1·09	1·12	1·15	1·18	1·21	1·26	1·31	1·36	1·41	1·46	1·51	1·57	1·63	*2·6*
1·07	1·08	1·09	1·11	1·14	1·17	1·19	1·21	1·25	1·30	1·35	1·39	1·43	1·48	1·53	1·59	*2·7*
1·17	1·15	1·16	1·17	1·18	1·19	1·21	1·23	1·27	1·31	1·35	1·39	1·43	1·47	1·52	1·58	*2·8*
1·33	1·28	1·26	1·26	1·25	1·25	1·25	1·27	1·29	1·32	1·35	1·39	1·43	1·47	1·52	1·57	*2·9*
1·53	1·47	1·42	1·38	1·36	1·34	1·34	1·34	1·35	1·36	1·38	1·40	1·43	1·46	1·51	1·56	*3·0*
1·85	1·72	1·62	1·56	1·52	1·49	1·46	1·44	1·43	1·43	1·43	1·44	1·45	1·47	1·51	1·57	*3·1*
2·34	2·13	1·96	1·83	1·75	1·68	1·62	1·57	1·54	1·52	1·51	1·50	1·50	1·51	1·54	1·58	*3·2*
3·03	2·69	2·44	2·23	2·06	1·94	1·85	1·77	1·72	1·67	1·63	1·60	1·59	1·58	1·59	1·61	*3·3*
4·08	3·50	3·06	2·74	2·49	2·29	2·14	2·02	1·92	1·84	1·78	1·73	1·70	1·68	1·68	1·69	*3·4*
5·72	4·75	4·12	3·56	3·15	2·83	2·59	2·38	2·22	2·09	1·99	1·91	1·85	1·81	1·79	1·79	*3·5*
8·85	6·90	5·71	4·79	4·15	3·68	3·22	2·90	2·67	2·49	2·31	2·20	2·09	2·00	1·96	1·92	*3·6*
14·2	10·4	8·22	6·69	5·70	4·81	4·17	3·67	3·31	3·05	2·81	2·62	2·45	2·29	2·19	2·10	*3·7*
24·9	18·1	13·8	10·1	8·16	6·71	5·76	4·92	4·28	3·84	3·48	3·16	2·91	2·68	2·50	2·36	*3·8*
65·5	36·5	21·7	15·8	12·2	10·0	8·17	6·75	5·75	5·00	4·43	3·93	3·52	3·18	2·91	2·72	*3·9*
242	86·6	48·5	30·0	20·9	15·4	12·0	9·61	7·86	6·60	5·67	4·90	4·34	3·88	3·47	3·19	*4·0*
—	374	127	62·9	38·3	26·0	18·3	14·4	11·5	9·15	7·53	6·45	5·66	4·98	4·38	3·80	*4·1*
—	—	—	200	91·0	51·6	32·7	23·4	17·4	13·4	10·6	8·70	7·50	6·48	5·52	4·61	*4·2*
144	478	—	—	314	112	70·0	42·9	29·1	21·3	16·1	12·7	10·3	8·55	7·24	6·25	*4·3*
62·8	135	—	—	—	580	192	93·9	55·8	37·1	26·5	19·8	15·4	12·1	9·74	8·26	*4·4*
42·8	68·3	119	280	—	—	—	286	126	72·8	46·4	32·2	23·9	18·3	14·6	11·8	*4·5*
32·3	44·7	62·7	99·6	240	742	—	—	—	181	91·0	58·9	41·0	30·0	22·3	16·9	*4·6*
26·2	33·8	46·1	68·0	105	240	532	—	—	—	260	128	76·7	50·4	36·0	26·6	*4·7*
21·6	27·1	35·0	47·3	66·8	104	182	413	—	—	—	403	172	99·3	63·0	44·8	*4·8*
18·9	22·6	26·9	33·7	44·0	61·5	84·2	115	337	—	—	—	—	249	140	80·8	*4·9*
17·8	20·7	24·6	30·1	37·8	48·9	66·0	97·0	157	286	—	—	—	—	—	172	*5·0*
17·2	20·0	23·0	27·1	32·5	40·6	51·5	69·2	99·8	147	253	559	—	—	—	—	*5·1*
17·3	19·3	21·7	24·8	29·5	35·4	43·1	54·7	70·6	94·6	138	216	—	—	—	—	*5·2*
18·0	19·7	21·4	23·9	27·1	31·5	37·0	44·2	54·5	69·3	93·2	132	205	380	—	—	*5·3*
19·6	20·5	21·6	23·3	25·6	28·6	32·8	38·0	44·8	54·0	68·6	94·0	130	185	—	—	*5·4*
22·3	22·0	22·5	23·6	25·2	27·5	30·5	34·3	39·4	45·4	54·7	67·3	86·5	116	169	275	*5·5*
—	—	—	24·5	25·8	27·6	29·8	32·2	35·6	39·6	44·8	51·4	63·2	83·0	118	168	*5·6*
—	—	—	26·8	27·8	28·8	30·0	31·5	33·8	36·5	39·8	44·4	53·1	67·0	85·2	116	*5·7*
—	—	—	30·7	30·8	31·0	31·3	32·0	33·4	35·3	38·2	42·2	48·4	56·6	69·9	87·9	*5·8*
—	—	—	38·4	36·0	34·4	33·5	33·1	34·1	36·0	38·7	42·2	47·0	53·1	61·9	74·8	*5·9*
—	—	—	50·4	42·5	38·3	36·8	36·4	36·5	37·9	40·0	43·0	46·6	51·3	57·8	66·2	*6·0*
—	—	—	—	—	—	—	41·0	41·0	41·6	42·7	44·6	47·2	50·3	55·0	61·5	*6·1*
—	—	—	—	—	—	—	48·2	47·0	46·1	46·0	46·7	48·0	50·0	53·5	58·5	*6·2*
—	—	—	—	—	—	—	57·3	54·0	51·6	49·8	49·1	49·4	50·0	52·5	56·8	*6·3*
—	—	—	—	—	—	—	79·4	66·6	58·6	54·2	51·8	51·2	50·8	52·5	55·5	*6·4*
—	—	—	—	—	—	—	128	84·0	66·9	59·9	55·9	53·9	53·1	53·6	55·4	*6·5*
—	—	—	—	—	—	—	—	—	—	—	—	—	57·4	57·0	56·7	*6·6*
—	—	—	—	—	—	—	—	—	—	—	—	—	63·5	61·5	59·2	*6·7*
—	—	—	—	—	—	—	—	—	—	—	—	—	73·6	67·8	63·5	*6·8*
—	—	—	—	—	—	—	—	—	—	—	—	—	97·2	77·7	70·5	*6·9*
—	—	—	—	—	—	—	—	—	—	—	—	—	134	96·0	82·0	*7·0*

β_2

MISCELLANEA.

I. Dr Galloway's "Canary Breeding."

By C. B. DAVENPORT.

In a paper published in *Biometrika*, Parts I and II of Vol. VII, Dr Galloway has criticised a "recent memoir" by me, "especially the material used" and the "use of terms." Criticism is always to be sought for by scientific workers and is usually beneficial to the progress of science; and, just as criticism is good, a rejoinder is often helpful to afford the original author, in the light of that criticism, opportunity to state any necessary change of conclusions or to make clear any difficulty which the critic has encountered. Accordingly I feel it to be my duty, although a not altogether agreeable one, to take the time and space for a rejoinder.

The principal points of criticism that Dr Galloway makes are (1) that I should "have selected the original stock with much greater care" (p. 2); (2) that my "sole criterion of a perfect crest...seems to be absence of a bald occiput" (p. 2); (3) that "yellow does not mean merely absence of dark feathering, as in Davenport's paper, but a particular quality of feather which may occur in a green, variegated or clear bird"; (4) that my statement that hybrids with the yellow canary "frequently show more or less of the canary yellow" is to be contradicted; and (5) that in my green × "yellow" matings, my "green" is not green but variegated and my "yellows" are also variegated. Besides these criticisms there are some points of difference in experimental results, namely; (*a*) in the offspring of "variegated" × clear green, or variegated (pp. 3, 4, 26), (*b*) in the mating of two crests, and (*c*) in the absence of homozygous crests. Finally, I shall be glad if I can assist the author and science by a few criticisms of some new points in his paper.

Of the five criticisms it will be observed that four deal with definitions and one with the material used by me. The latter point may be considered first. The criticism on my original stock is not directed towards any concealment on my part, for the data of origin, so far as known, are given and one or more paintings of original stock published. It is rather directed towards the unfitness of races that are bred for song to give evidence concerning inheritance of colour and towards the fact that my birds were not "pure-bred." I am somewhat embarrassed in replying to this criticism since the author does not define what he means by pure-bred. I know what notion I associate with "pure breeding," namely, continual self-fertilization, such as occurs in sweet peas and many other plants, but obviously that kind of pure breeding is unknown among canaries. Some fanciers, I know, mean by pure breeding mating inside the same race or variety; but this definition is hopelessly vague since there is no agreement as to the limits of relationship or similarity compatible with these terms. A few fanciers mean by "pure-bred" birds, birds having a characteristic *a* all of whose ancestors for several generations have the same characteristic *a*. For such, the offspring of Crested Norwich × Crested

Manchester Coppy would be "pure-bred." This meaning of "pure-bred" is at least clear; but its utility may be doubted; nor does such "pure breeding" insure uniformity of progeny, since even if the crested parents have had only crested ancestors for many generations they may still throw plainheads. As a matter of fact, all philosophical breeders of experience realize that the fetish of "pure breeding" is a will-of-the-wisp, and for the most part a hopelessly obscure, and unsatisfactory conception. Practically the experienced breeder need not greatly care (within limits) about the ancestry of his birds. A little experience in breeding will give him greater assurance as to the character of the germ plasm than the affidavits of the fancier. To return to my stock, they were live, healthy and mostly fecund birds,—*canary* birds, no doubt. That is about as far as it was necessary to go. If I offered the additional information that they might be classified as "Harz" or "Norwich," that might help a possible critic; but I laid little stress on the statement and, from my experience, think it unimportant. I would defy anybody to *classify* my canaries now, but nevertheless I know more about their germ plasm than most punctilious fanciers do about that of their pure breeds. And a knowledge of performance of germ plasm, not capacity for satisfying the conditions of the "Standard," is what I have worked for.

As for my terms, it is the privilege of an author to use them as he sees fit provided he defines them clearly. Not having the fortune to live at Aberdeen and to mingle with Scotch fanciers, I have, no doubt, defined some terms differently from my critic. That was not only harmless but necessary. I specifically defined "imperfect" crest as I was about to use it for the immediate purpose of the Table and four paragraphs of Text (p. 11) as "more or less bald on the occiput." I cannot find that my critic anywhere gives his definition of the term. He merely says (p. 2) "this bald spot accompanies most of the perfect crests of the present day." Our use of the word "perfect" in the two sentences differs—that is clear and sufficient.

Next, my critic objects to my sense of the word "yellow." I was not unaware of the use of "Yellow" by most English fanciers in a technical sense. Thus Blakston says (p. 94), "we speak of a Yellow Green or a Buff Green, a Yellow Cinnamon or a Buff Cinnamon, when it is patent that neither of these colours can be yellow or buff in reality; and the words, therefore, taken in their general application, are technicalities." Now I was not writing for English fanciers merely, I was writing for men of science without regard to nationality, and to have used "Yellow" in the fanciers' sense without a long explanation would have led to confusion. I preferred, in common with some other authors in English (and as clearly stated on page 14 of my book), to use *Jonque* and *Mealy* instead of "Yellow" and "Buff." That left me free to use yellow in what Blakston calls the *real* sense; the sense also in which the German fanciers, as exemplified by Karl Russ, use it when they speak of Hochgelbe and Strohgelbe. If I had intended to use yellow as a technical group-name I should certainly have printed it in capitals, as Blakston does, and as I print "Buff Cochin." As for the assertion that my definitions of "jonque" and "mealy" are inadequate, referring to colour merely, instead of including the form of the feathering and that of the whole body of the bird, I may say merely that not having been acquainted with my critic I could not have known his use of the terms; that my definitions were based on and are practically identical with those of Blakston's great English classic (p. 94). In so far as they do not include form of the body of the bird they are devoid of the probably fantastic associations of a special group of fanciers in the vicinity of Aberdeen. At least, I find in my hybrids no necessary correlation of jonque and mealy plumage colour with the form of the body of the bird.

As for the *emphatic* denial (p. 4) of my statement that hybrids of the yellow canary "frequently show more or less of the canary yellow," I need only ask any intelligent reader if the assertion is contradicted by my critic's statement that "the great bulk" of such hybrids are dark. "Frequently" implies less than half of them light; "the great bulk" more than half dark. I might willingly have added to my book the same words as my critic, except to avoid

tautology. Finally, my critic's categorical statement (p. 32) that my "greens" and "yellows" are both variegated (in his sense) is as gratuitous as it is unscientific and false as a matter of fact.

We may next consider the differences between Dr Galloway's experimental results and mine.

(*a*) That, as stated on pages 3, 4, my critic should have got 15 unmottled to 5 mottled* offspring from non-crested birds, where 1 : 1 was expected is no argument against Mendelian results, since the numbers are insufficient. In my breeding, single families showed such deviation from expectation, but the totals of 32:30 revealed the true significance of the mating. As to my critic's divergent proportions with *crested* birds no conclusion can be drawn without a knowledge of his definition of "variegated." I suspect he includes in this category birds with pigmented crests ; I did not. There is an interesting correlation between crest and head pigment in my birds and so head pigment was disregarded in crested birds, and such as had no other pigment were called "clears" or "yellows."

(*b*) Dr Galloway finds that crest × *crest-bred plainhead* gives an equal number of crested and crestless young. As I showed in my paper this result follows when plainheads of *any* origin are mated with heterozygous *crests*. He finds that in the offspring of two crested parents, crests are to plainheads as 2:1. This is a very unanalytical grouping. The results are 1 : 0 or 3:1 according as parents are homozygous or heterozygous in crest.

(*c*) Dr Galloway finds no case of crest × plain that produces crests only. This is not strange, as at least twice as many heterozygous as homozygous crested are to be expected.

Passing now to a criticism of other points in Dr Galloway's paper one might first wish for a clearer table of matings and progeny, with individual numbers so that one could trace results in cases where the same bird is used several times in different matings. This information the appendix does not readily, if at all, afford. Secondly, one can only regret that, as so commonly the case with scientifically untrained writers, so much stress is laid on crude speculation, in this case as to the origin of domestic races of birds through a "cinnamon sport." In its application to poultry, certainly, the cherished theory is not appealing. And why is the commonest of Hervieux' canary colour types, translated literally "gray" instead of technically "green" (p. 13), said to be closely allied to the cinnamon canary (p. 18) ; and even given as a sort of synonym of the latter (p. 30) ?

At the end of this polemic it is a pleasure to speak of the valuable data in this paper of Dr Galloway's, the account of the inheritance in eye colour with its confirmation of the Bateson-Punnett theory and the conclusion that two Mealies cannot produce Jonques. With the general theory that the races of domestic animals have arisen by mutations the writer finds himself quite in accord with Dr Galloway. One can only wonder that, where the Mendelian theory is so often appreciated and supported, the author should occasionally fail to grasp it. And, best of all, it is a pleasure to see the increasing catholicity of the active editorship of *Biometrika* in accepting such a paper for publication†.

* Mottled, as used by me, is *not* the equivalent of variegated as used by Blakston and, apparently, by Dr Galloway. It includes "light variegated," "heavily variegated" and "marked."

† [This statement of Dr Davenport's needs an editorial comment. No paper dealing with heredity from the Mendelian standpoint has ever been refused by this journal, although such papers would be declined if they were considered inadequate experimentally or theoretically. Biometric papers British, American, and foreign have been rejected on these grounds. K.P.]

II. Canary Breeding. A Rejoinder to C. B. Davenport.

By A. RUDOLF GALLOWAY.

In writing my "Canary Breeding," one of the chief objects I had in view, was to secure some sort of agreement between scientific men and fanciers; for the attainment of such an end would undoubtedly result in an increase of knowledge, and would greatly benefit both parties.

Dr Davenport's frequent references to fanciers and their terms, and his acknowledgment of Blakston's *Great English Classic*, compel me to judge his statements from the fancier's point of view, as well as from that of the scientific man.

From the scientific standpoint only, his statements might be more consistent, though in all probability valueless; from the fancier's point of view, which is pre-eminently that of *his* great authority Blakston, it is easy to show that his interpretation of terms is extremely faulty:—

In his Rejoinder, he makes the startling admission that he has in his tables classed dark crested birds as "clears or yellows." The very thought of this is enough to make Blakston turn in his grave, for the following is his (Blakston's) definition of a clear bird:—

"And here we draw the line" (at dark underflue), "and define 'Clear' to mean not having the remotest tinge of dark colour in quill, flue, or feather; birds from which every trace of the green has been eliminated; which have a clear, silky, snowy-white underflue and show by it that they have arrived at the goal" (Cassell's *Canaries and Cage Birds*, p. 100).

Such an interpretation of standard nomenclature is unwarranted, and indeed incomprehensible in one who regards Blakston with reverence and authority. It is needless to enter further into the meaning of the term "variegation," which Blakston so fully and clearly explains (pp. 95–101), for it is evident that Davenport's interpretation of it is his own conception (as indeed he admits), and consequently his results cannot be accepted by the initiated.

Another extract from the Rejoinder must be given in full:—

"As for the assertion that my definitions of 'jonque' and 'mealy' are inadequate, referring to colour merely, instead of including the form of the feathering and that of the whole body of the bird, I may say merely that not having been acquainted with my critic, I could not have known his use of the terms; that my definitions were based on and are practically identical with those of Blakston's great English classic (p. 94). In so far as they do not include form of the body of the bird, they are devoid of the probably fantastic associations of a special group of fanciers in the vicinity of Aberdeen."

Nevertheless, if Dr Davenport will turn to p. 101 of the *Great English Classic* (of which, he will be glad to hear, a new edition will shortly be published—the original one having served its time and generation *) he will read as follows:—

"The feathering on the Buff birds is much denser than in the Jonques, the under-flue being very thick and long, as the fancier will be able to observe when he makes his first essay in washing. Altogether the Buff bird is *built on a larger and stouter scale*, and is, in every respect, the more *lusty* bird of the two."

It is hopeless for Dr Davenport to escape from his erroneous use of the word "yellow," for certainly Blakston never used it as he does, and if I am not greatly mistaken his appeal

* Cassell's *Canaries and Cage Birds* was published in London, Paris, *New York* and Melbourne, some *thirty* years ago.

to German fanciers is equally at fault, for 'hochgelb' and 'weissgelb' mean undoubtedly jonque and mealy (yellow and buff). In support of this let me give a note received on Dec. 1st, 1908, from that well-known fancier and scientific writer C. L. W. Noorduijn* of Groningen :—

"I mean by buff-yellow, light or whitish yellow (mealy) (weissgelb), in contra-distinction to clear or orange yellow (hochgelb)."

But even if we allow Dr Davenport to have his use of the term yellow, the mere fact that his matings take no account of the rudimentary distinction between the jonques and mealies, is amply sufficient to seriously detract from their value.

The following statements on Davenport's pages 6 and 7 must also be corrected :—

(1) That Hervieux in 1713 gives only 10 varieties (p. 7).

(2) That during the first half of the eighteenth century, the number of colour varieties was greatly increased, since Hervieux, in his edition of 1766, recorded 29 colour varieties (p. 7).

And (3) that the first edition of Hervieux was published in 1705 (p. 6).

The facts are as given in my "Canary Breeding" :—

(1) That there is no 1705 edition—the first one appearing in 1709.

(2) That in the 1709 edition, 28 varieties are mentioned.

This list also disproves the conjecture "that the frizzled characteristic is probably relatively recent, as it is relatively uncommon" for it occurs among the 1709 varieties.

It is perhaps superfluous to refer to the meaning of the word "frequently"; suffice it to say that the occurrence of 1 clear mule in 526 (see table on p. 4 of my "Canary Breeding") cannot be included under that category.

In the original draft of my paper, I found fault with Dr Davenport for translating the French word "gris" as green in his memoir (p. 7). This grumble was found wanting in the proof sheets, and I now refer to it only because in the Rejoinder the author finds fault with me for translating the same word "literally gray" instead of "technically green." The technicalities of science are worthy of reverence, but when they transform the colour grey into green, then there is something wrong, which should be corrected.

In reply to a query in the Rejoinder, I may state that grey and cinnamon sports are stated to be closely allied by me, because of my experience in keeping and breeding them—as may be seen throughout my paper (e.g. greenfinch family, p. 32).

With regard to the statement that I have not defined imperfect crest, it should not be necessary for me to refer to a whole paragraph on p. 2, where this is fully explained, nor to a special engraving on p. 24, which beautifully illustrates the modern crest of various degrees of perfection.

My "unanalytical grouping" of the progeny of double-crests (2 crests to 1 crest-bred plain-head) is due to the fact that I could not assume the existence of homozygous crests which I had not found.

Dr Davenport's tirade against pure-breeding is extraordinary as coming from the pen of an ultra-Mendelian, and from a follower of the great Blakston, who devotes the whole of Chapter XIII. to the discussion and praise of Pedigree breeding.

Let me again quote from the Rejoinder. "A few fanciers mean by 'pure-bred' birds, birds having a characteristic *a* all of whose ancestors for several generations have the same character-

* Noorduijn discredits Davenport's Mottling Theory, criticizes adversely his inferences from Hervieux and Russ, and completes the similarity to my own position by showing that the frilled canary is not "probably relatively recent, as it is relatively uncommon"; he likewise disagrees with Davenport's crested experiments. (*Kritische Besprechungen und Referate. Archiv für Rassen- und Gesellschafts-Biologie*, Jahrg. 1909, S. 394.)

istic *a*. For such, the offspring of Crested Norwich × Crested Manchester Coppy* would be 'pure-bred.'" As the characteristic *a* must mean the crest and coppy (which however are very dissimilar) and as the rule in Crests and Coppies is to mate a crested bird with a crest-bred plainhead, a coppy with a coppy-bred plainhead, it is difficult to comprehend the sort of fancier to which reference is made.

I must state, however, that our respective interpretations of the term "pure-bred" have nothing to do with the point at issue, which was my criticism of a result of Dr Davenport's—on the ground that pure-bred birds had *not* been used—purporting to overturn one of Russ', in which he (Russ) expressly stipulated that pure-bred birds were necessary. (See Davenport's quotation, p. 15.)

Concerning the "crude speculation" of my evolution theory, I sincerely hope that it will form the subject of serious discussion by competent authorities. I have many more facts in its support than could be produced in a paper which had to include such an amount of spade work.

With one statement in the Rejoinder, I am in complete agreement with Dr Davenport, viz. "I would defy anybody to classify my canaries now." He has succeeded in proving by his so-called scientific method, not only that a crestless bird is a perfect crest, but that it has a more perfect crest than three-fourths of the existing prize crested birds. By some ingenious allelomorphic scheme he might as easily prove that all bipeds were quadrupeds. From his false assumptions both in the matter of crest, and plumage-colour, it is quite impossible for him to arrive at any scientifically correct, or practically useful conclusion.

III. Inheritance in Canaries: A Study in Mendelism.

By DAVID HERON, M.A.

In 1908 there appeared a paper† on *Inheritance in Canaries*, by Chas. B. Davenport, Director of the Station for Experimental Evolution, Carnegie Institute of Washington, in which the author discusses the inheritance of form of crest and plumage-colour in canaries and concludes that those characters follow Mendelian rules.

These results are accepted without criticism by Bateson‡, and although Galloway§ has offered some criticism, it seems desirable that further attention should be called to the methods used in this paper.

It will not however be necessary to consider Davenport's discussion of the inheritance of plumage-colour; his treatment of the inheritance of form of crest on pages 8 to 13 and the general list of matings in Table E, p. 24, so far as it deals with crest-form, will be found quite sufficient for the present purpose, i.e. to measure the scientific weight of this paper.

Before considering his results a strong protest must be made against the very defective condition in which they have been presented. From the material we can only arrive at a lower limit to the number of blunders in those few pages (in all 114 lines of text and four Tables) by noting those cases in which the tables are self-contradictory, e.g., when a bird is said to be crested in one table and non-crested in another.

* A redundant term!

† *Carnegie Institute of Washington Publication*, No. 95. Papers of the Station for Experimental Evolution, No. 10.

‡ *Mendel's Principles of Heredity*, pp. 37 and 43.

§ *Biometrika*, Vol. VII. p. 1 et seq.

In Table E, on p. 24, the full list of matings is given. As it stands, it is far from satisfactory, and the list of errors in the statement of form of crest which can be detected from a study of this table alone, is a large one. Only 66 matings in all are given.

In Experiments 501, 509, 616 and 723, a reference to Exp. 624 should be to Exp. 623. In Exp. 714, a reference to Exp. 625 should be to Exp. 624; Exp. 625 does not occur in this Table at all. In Exps. 716 and 725, a reference to Exp. 623 should be to Exp. 622. In Exp. 702, there is a mating with a bird which has no number at all. It is said to be a crested bird but there is no indication whether the crest is perfect or imperfect. Similarly no statement as to the presence or absence of baldness is made about Birds Nos. 50 (used twice), 269, and 240. Bird No. 110 used in Exp. 729, is not marked either for crest or baldness; it comes from Exp. 604 but it does not occur in the columns in which crest and baldness are indicated although it is marked "Yellow" in the colour columns. To balance matters, however, Bird No. 192 in this Experiment (604) is given in the columns for crest but is dropped in the columns for colour, while Bird No. 112 is entered twice in the colour columns. In Exp. 703, Bird No. 67 is said to come from Exp. 10; it really comes from Exp. 505. In Exp. 703 also, three birds are said to have died young when considering crest and four when dealing with colour. In Exp. 503, Bird No. 40 comes from Exp. 405, not from 505 as stated. In Exp. 709, Bird No. 143 comes from Exp. 609, not from 607 as stated. In Exp. 405, Birds Nos. 39 and 40 are said to have "crest present"; in Exps. 503 and 511, No. 40 is said to have "crest absent," while in Exp. 614, No. 39 is said to have "crest absent." As an example of an error of omission, there appears in Table II (Exp. 717), a mating between two birds which are not numbered; this mating does not occur in Table E at all.

Disregarding this serious list of blunders, however, we may now proceed to examine Tables I, II and III, and Davenport's deductions from them. Since those tables can only have been made up from the General List of Matings, Table E, we shall assume that Table E, in so far as it is not self-contradictory, represents the facts of the case*.

Dealing with presence or absence of crest, Davenport states (p. 8, 6 lines from bottom) that "crest is alternative in inheritance, for when crested and non-crested birds were paired, the offspring were either well crested or plain headed and there were no intergrades." Davenport here uses "non-crested" as equivalent to "plain headed." A few lines further on, however (p. 9, line 1), he says that Table I "gives an answer to the question whether non-crested heads are recessive to plain heads"; while in his conclusions on p. 23, he says that "crest is dominant over plain head." In the second of those three quotations "plain heads" ought to read "crested heads."

Turning now to Table I, we find again many blunders here.

For Exp. 514, read Exp. 509; for Exp. 613, read Exp. 614; for Exp. 614, read Exp. 615; for Exp. 615, read Exp. 616; for Exp. 623, read Exp. 622; for Exp. 624, read Exp. 623; for Exp. 711, read Exp. 712; for Bird No. 185 in Exp. 716, read Bird No. 186.

Exp. 619 gave 12 non-crested birds, not 11 as stated.

Further Bird No. 39 used in Exp. 614 (not Exp. 613 as stated) and said in Table I to have "crest absent," is said in Table E, Exp. 405, to have "crest present" and in Table E, Exp. 614, to have "crest absent." In Table I, however, the result of Exp. 405 is said to be two birds with "crest absent" and none with "crest present." In Table E, the result of Exp. 405 is said to be two crested birds and none without crest.

Similarly in Table E, Exp. 507 is said to give one bird with "crest present" (No. 71); in Table I however it is said to give one bird with "crest absent." This bird No. 71 is not used again, so further light cannot be thrown on the matter.

* Davenport states (p. 9, line 1) that Table I is "extracted from the general table."

Again Exp. 726 in Table E, is said to be a mating of non-crested birds and thus ought to be included in Table I, while the father of Bird No. 142 used in Exp. 723 is crested, not non-crested as stated.

Davenport states that out of 102 offspring of non-crested parents, all were non-crested. It is true that in this conclusion he is supported by Galloway but the point to be insisted on here is that this conclusion cannot be reached from his own data since, according to Table E, Birds Nos. 39, 40, and 71, were crested birds and came from non-crested parents.

Turning now to Table II, we find again an extraordinary number of blunders.

For Exp. 710*a*, read Exp. 711; for Exp. 713, read 714; for Exp. 515, read 513; for Exp. 625, read 624; for Exp. 712, read 713.

Further, the results of Exps. 703, 604, 704 and 720, are not in accordance with the results of those experiments given in Table E.

In Exp. 703, instead of the proportion of non-crested to crested birds, 3 : 7, read 3 : 6; in Exp. 604, instead of 7 : 1, read 7 : 2; in Exp. 704, instead of 0 : 7, read 0 : 5; and in Exp. 720 instead of 0 : 1, read 0 : 2.

In Exp. 714 (not 713 as stated), Bird No. 240 is said to come from two crested parents. According to Table E, however, No. 240 is one of the "original stock of whose ancestry, consequently, nothing is known directly." Similarly in Exps. 503 and 511, Bird No. 3 is said to come from a mating of crest and non-crest, while on p. 11 and in Table E it is said to be "original stock."

Davenport's method of dealing with those tables is equally faulty.

In part 1 of Table II he deals with matings between crested birds. Now crested birds, on the assumption made by Davenport, that crest is dominant over absence of crest, may be either DD or DR, and if DD be mated with DD or DR all the offspring should be crested, while if DR be mated with DR, three-fourths of the offspring should be crested.

It is important therefore that DD's should be distinguished from DR's. If any of the offspring of a pair of crested birds are non-crested then both parents must be heterozygous; but if all the offspring are crested, it by no means follows that one or both of the parents is homozygous. So long as only crested birds are produced we cannot distinguish between DD's and DR's. All we can do is to appeal to the laws of probability and estimate the chance that all the offspring of a pair of crested birds, which are really heterozygous, shall be crested.

Now Davenport, as the author of a book on Statistical Methods, must be familiar with the various probabilities involved in those cases. He says that "at least two birds are homozygous in crest, No. 12 which has produced nine young all crested and No. 79 which has produced 11 young all crested." One is not surprised to find that according to Table E, No. 79 has produced only nine young all crested and this correction must first be applied. Further he says that No. 126 which has produced four young all crested is "possibly homozygous"; and that No. 9 has produced 16 crested birds and one non-crested.

Let us consider the probabilities involved in those cases. What is the chance that a pair of DR's, i.e. crested but heterozygous birds, will produce "n" young all crested? In the long run they will produce three crested birds to one non-crested bird, and thus the chances that n, $n-1$, $n-2$, etc. out of n will be crested are the successive terms of $(3+1)^n$ out of 4^n. Thus the chance that all out of nine shall be crested is $(\frac{3}{4})^9 = 1$ in 13.

Now he actually gets in his experiments the proportion 16 : 1. What is the probability of such a combination arising? It is

$$17 \cdot \frac{3^{16}}{4^{17}} = 1 \text{ in } 23.$$

Also the combination three birds without crest out of three arises. The chance that this will arise is still more remote—1 in 64.

Now Davenport rejects the combination 9 : 0 which occurs once in 13 times while he finds among his material combinations whose chance of occurring is far more remote, and yet retains them !

Homozygous and heterozygous birds in such cases can only be distinguished by increasing the number of experiments, but in cases of doubt crested birds should be mated with non-crested birds as then the chance that all of nine for example shall be crested is only 1 in $2^9=1$ in 512. To get the same probability when mating two crested birds, we must obtain 22 offspring. Davenport does not seem to recognise that whenever all the offspring are crested, whether there be only two or as many as nine, there is a possibility of one or both parents being homozygous.

Davenport's failure to test adequately the gametic constitution of the parents renders any appeal to his totals useless and a similar criticism applies to part 2 of the same table. All we can say is that in Table II, part 1, there are some matings which are DR×DR and others which may be DD×DD, DD×DR, or DR×DR, and that conclusions based on methods such as these are of no value.

His treatment of the inheritance of baldness however is even more faulty, and in Table III, one is confronted at the outset with a series of blunders and mis-statements which are absolutely unparalleled. In Table III, there are given the results of 26 matings, and in the following cases the statements made are contradicted either in the same table or elsewhere in the paper.

In Table A, Exp. 617, Bird No. 84 should be No. 82; and in Exp. 620, Bird No. 83 has a "perfect crest," not "imperfect" as stated. Exp. 620 should therefore be included in Table C and not in Table A. In Table B, α, Exp. 505, Bird No. 34 is said to have a perfect crest; in Exp. 513, C, β, it is said to have an imperfect crest, while in Table E, Exp. 403, it is said to have a perfect crest. In Table B, α, Exp. 505, four of the offspring are said to be non-crested; only two are given in Table E. In the same experiment (No. 505) Bird No. 7 is said to be without crest; in Table C, β, it is said to have a perfect crest, while in Tables I. and E it is said to be without crest. In Table B, α, Exp. 625 should be No. 624.

In Table B, β, among the offspring of Exp. 705, there are only three birds with perfect crest and not five as stated. In Exp. 714, in the same table, there is said to be one bird with imperfect crest and six in all. In Table E, for this experiment there are four birds with perfect crest, one with crest absent, and two which died young. No bird is marked as having an imperfect crest. According to his own figures instead of totals of 11 perfect crests and four imperfect crests there are nine perfect crests and three imperfect crests together with seven birds which are without crests; such birds are in the same table considered as perfect crested birds, i.e. they are not bald. There are thus 16 birds with perfect crests and three with imperfect crests.

Further, no imperfect crested birds are *known* to have occurred in the ancestry of Birds Nos. 157 and 240, and to state as he does that such is "probably" the case is to beg the whole question.

In Table C, α, Exp. 508, there should be one bird without crest. In Exp. 604, Bird No. 62 has a "perfect crest," not an "imperfect crest" as stated. This mating is therefore wrongly included in the Table. In Exps. 608 and 703, Bird No. 67 should have a "perfect crest," not an "imperfect crest" as stated. These matings should therefore be included in Table B and not in Table C. In Exp. 703, there are only two birds with perfect crest, not three as stated. In Exp. 704 there are only three birds with perfect crest, not five as stated. In Exp. 711 there are two birds with perfect crest and none with imperfect crest, not one of each as stated.

In Table C, β, in each of Exps. 513 and 621 (not 622 as stated) one bird without crest is dropped.

The climax is however reached in dealing with two matings between Birds No. 6 and 9.

In Table C, α, Exp. 510, this is said to be a mating of DR×RR; in Table C, β, Exp. 504, to be a mating of DD×RR. In the first case it is said to give four imperfect crested birds and none with perfect crest; in the second case it is said to give two perfect crested birds and one bird without crest. In the first case No. 6 is said to have an imperfect crest and No. 9 a perfect crest; in the second case No. 6 is said to have a perfect crest and No. 9 an imperfect crest. On p. 11, line 32, No. 9 is said to have a perfect crest and No. 6 an imperfect crest. The extraordinary versatility of these two birds is decidedly embarrassing and one is somewhat surprised to find that the tables in which such blunders occur are headed "Matings to Test Inheritance of Imperfect Crest." Davenport makes a similar blunder in dealing with two matings between Birds Nos. 7 and 34. It has already been pointed out that No. 7 has at one time no crest and at another time a perfect crest, and that No. 34 has at one time a perfect crest and at another time an imperfect crest. So also the mating 7×34 in Exp. 505 is said to be one of DD×DD or DD×DR, and in Exp. 513 the same mating is said to be one of DD×RR!

Again, Table B is divided into two sections α and β. Section α deals with matings DD×DD and DD×DR, while section β deals with the mating DR×DR. But the distinction is purely arbitrary; as has already been explained if a mating of birds with perfect crest results in one bird with imperfect crest, then the mating must be DR×DR but further we cannot go. The same criticism applies to his division of Table C into two parts.

Various conclusions from Table III are given on p. 13, e.g. "Table III shows that when two imperfect crested birds are mated, the offspring have imperfect crests (Table III, A). This indicates that absence of occipital feathering is recessive to its presence." He gives those matings in Table III, A, but omits Exp. 403 which is a mating between Birds Nos. 21 and 13. These birds are said on p. 11 to have imperfect crests. The result of Exp. 403 is, one bird with crest absent and one with crest present and perfect (No. 34). It has already been pointed out that this bird is stated twice to have a perfect crest and once to have an imperfect crest. The balance of probability being in favour of its having a perfect crest, the conclusions drawn are not justified and are flatly contradicted by Davenport's own material. Further, Exp. 620 should be deleted from Table A and placed in Table C, since Bird No. 83 has a "perfect crest" and not an "imperfect crest" as stated.

Again on p. 13 we find "when both parents have a perfect crest (being therefore DD or DR) all the offspring have the perfect crest (Table III, B, α)." This of course would only be the case for the matings DD×DD or DD×DR as stated in the Table, but the heading of Table III, B is "Neither parent has imperfect crest" and the table actually contains an incomplete list of matings in which at least one parent has a perfect crest and in which one parent has (in seven cases out of nine) no crest at all. These statements are all consistent with each other if non-crested birds are considered as having perfect crests, but Davenport excludes them in counting the offspring. It is quite justifiable for Davenport to group together birds with perfect crest and birds with no crest since neither according to him have the bald spot on the occiput, but, if he does so in considering the matings, he must also do so in counting the offspring, and this he fails to do. He actually leaves out of account altogether 39 birds out of 112.

It is impossible to tell what he intended to include in this table; he gives two cases of mating of perfect crest with perfect crest and seven cases of perfect crest with absence of crest. But if this is what he means, the list is far from complete. Assuming that his statements as to perfect crest, imperfect crest or absence of crest as given in Table E are correct, the following experiments must be added, Nos. 402, 502, 513, 604, 606, 608, 611, 701, 703, 709, and perhaps 514 and 702.

In those last two experiments it is not stated whether the crest is perfect or imperfect in the case of at least one of the parents, Birds Nos. 50, 269, and the mate of 269 which has no number.

Of this list, Nos. 513, 604, 608 and 703 are given in Tables III, C, α and β, but their inclusion is in flat contradiction to Table E.

Table III, C, α, is stated on p. 13 to consist of matings between perfectly feathered individuals that are probably hybrids between dominants and recessives, i.e. DR's and imperfectly feathered consorts. The heading of the Table is, however, "One Parent has Imperfect Crest." According to Table E, however, Bird No. 34 has a perfect crest and Exp. 513 should thus be included in Table B. As before, however, "absence of crest" is considered as equivalent to "perfect crest" in dealing with the parents, but birds without crest are rejected in counting the offspring.

In Table III, C the following should also be included: Exps. 511, 601, 620, 621 and perhaps 514, 613, 702 and 714, but absence of information in Table E leaves the matter in doubt.

The gametic scheme on p. 13 which assumes that in the crest there are two pairs of allelomorphs is not justified. It required the presence of bald plainheads, and out of Davenport's 154 non-crested birds none have been found to be bald. Galloway also says (*Biometrika*, Vol. VII. p. 25) that no bald plainheads have occurred in his experience.

The material used by Davenport is also unsatisfactory. Of the birds used in those experiments some are said to have been of the "Norwich" type, and in Plate I, Fig. 2, in Davenport's paper, is given an illustration of a crested Norwich canary. This is however so far removed from what is known as a Norwich crest that it seems desirable to consider the point further. I have examined carefully every Norwich canary exhibited at the 1910 exhibitions of the Scottish National Cage Bird Society at Edinburgh and of the London and Provincial Ornithological Society at the Crystal Palace, some hundreds in all, and saw no bird at all like that shown in Fig. 2. Such birds sometimes appear but they are rare and are certainly not representative of the class.

There are three well-marked types of Norwich canaries: the Norwich plainhead, the crest-bred plainhead, and the Norwich crest. These three birds are quite distinct and cannot be confused by anyone who has ever seen them. The crest-bred plainhead and the Norwich crest are usually bred together, but the Norwich plainhead is never bred with the other two. Now it is not clear whether Davenport's non-crested birds are plainheads or crest-bred plainheads or indeed whether he is aware of the distinction between these two types, but that they are quite distinct can be seen clearly from the illustrations given here. In Fig. 1, we have a typical Norwich plainhead. It is very neat in appearance and shows no trace of overhanging eyebrows and offers a strong contrast to Fig. 2 which is a crest-bred canary. In this bird the head feather is very dense, and when it is turned forward with the finger it reaches right over the tip of the beak. The illustration also shows the typically heavy eyebrows of this class.

In Fig. 3 is given an illustration of a Norwich crest. The crest is very regular and radiates from a well-defined centre; the eyes and beak are completely covered by the crest and it is clear that this bird has very little in common with the "Norwich crest" illustrated by Davenport.

It is obviously of the highest importance that the existence of these *three* distinct types should be recognised. In respect of crest the crest-bred plainhead occupies a distinctly intermediate position and cannot be classed either with the Norwich crest or with the Norwich plainhead.

It has thus been shown that every conclusion made by Davenport can be proved to be false from a study of his own material; that if a fact has to be stated twice the one statement is flatly opposed to the other and that blunder is heaped on blunder until patience is exhausted.

Yet such work is accepted as showing that Mendelian rules apply to Canaries! (See Bateson, *loc. cit.* pp. 37 and 43.)

Before the cautious man of science can be convinced of the truth of Mendelism, it will be necessary that in Mendelian writings a much higher standard of observational and statistical accuracy should be employed.

TYPES OF NORWICH CANARIES.

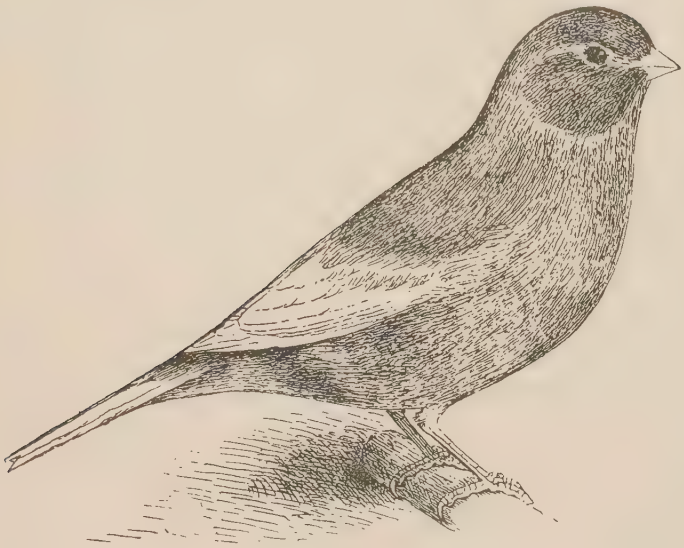


Fig. 1. Norwich Plainhead.

Fig. 2. Crest-bred Norwich Plainhead.

Fig. 3. Norwich Crest.

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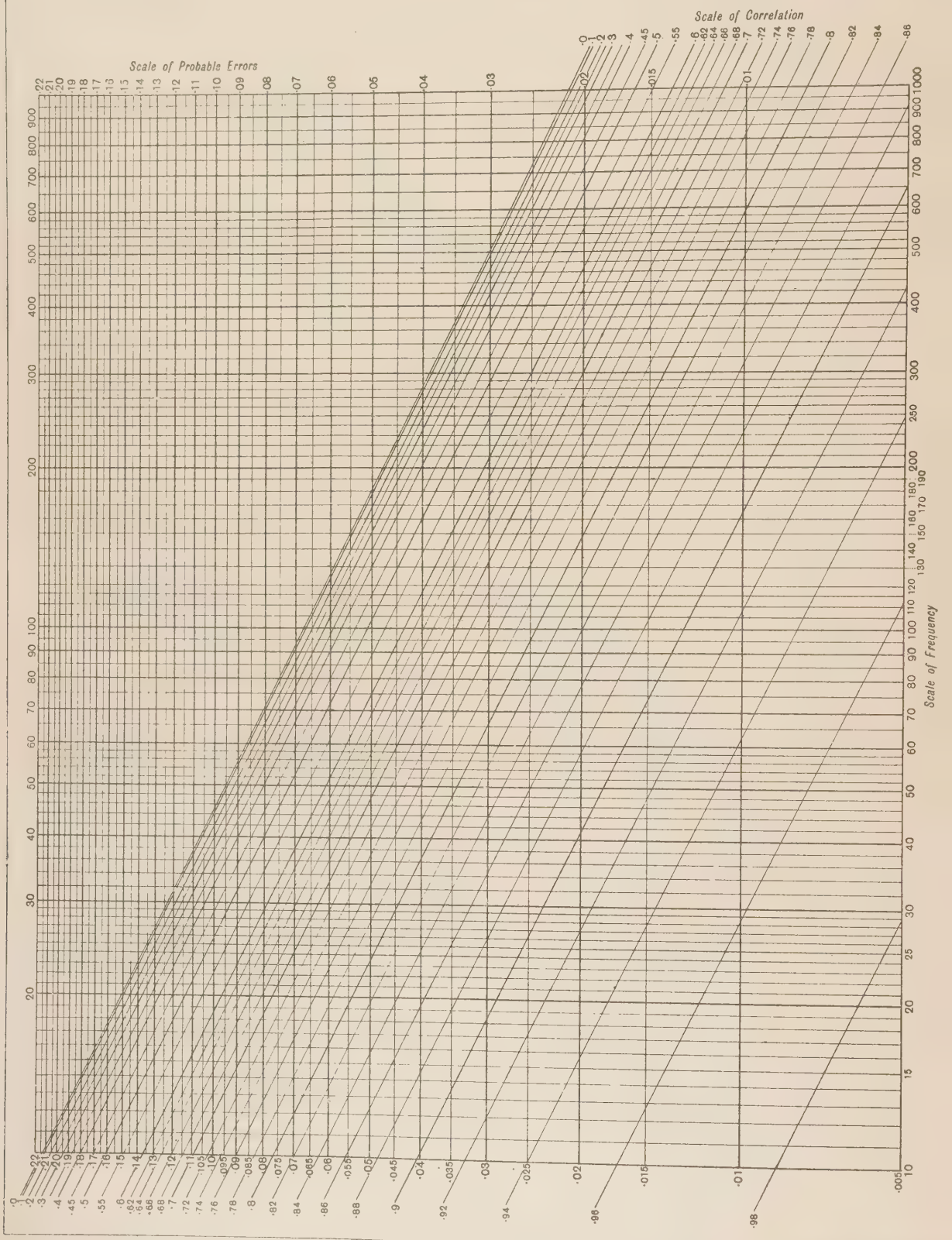

Abac for determining the Probable Errors of Correlation Coefficients.

BIOMETRIC NOTES.

I. An Abac for determining the Probable Errors of Correlation Coefficients.

By DAVID HERON, M.A.

The probable errors of correlation coefficients are so often required that the accompanying abac which enables them to be determined at once will save considerable labour. From this abac the probable errors can be read off correct to at least two decimal places; greater accuracy is seldom required.

The principle on which the abac is constructed is simple. If we take the well-known formula for the probable error of a correlation coefficient

$$E_r = \frac{\cdot 67449\,(1-r^2)}{\sqrt{n}},$$

where r is the correlation and n the number of observations, and express it in logarithmic form, we get

$$\log(E_r) = \bar{1}\cdot 8289755 + \log(1-r^2) - \tfrac{1}{2}\log n.$$

For any constant value of r there is thus a linear relationship between the logarithms of E_r and n. Hence by plotting E_r and n on logarithmic scales and ruling lines for a sufficient number of values of r, we can find E_r from r and n without difficulty.

The use of the abac can best be illustrated by an example. Let the correlation between two variables be ·55 and the number of observations 160; then to find the probable error of r, we must read along the perpendicular line from the number 160 on the scale of frequency until it crosses the diagonal line representing a correlation of ·55. The position of the point of intersection of these two lines is then read, by aid of the vertical lines, on the scale of probable errors and the value ·037 so obtained.

After the diagram had been drawn in pencil, the whole of the laborious work of ruling in the lines in ink and preparing the diagram for reproduction was undertaken by Miss H. G. Jones, and I have to thank her most heartily for her careful work.

II. On the Probable Error of a Partial Correlation Coefficient.

By DAVID HERON, M.A.

In a paper "On the Theory of Correlation for any Number of Variables, treated by a New System of Notation,"* G. Udny Yule has suggested that the probable error of a partial correlation coefficient, giving the correlation between two variables for a constant value of a third, is of the same form as the probable error of the direct correlation between any two variables, but his proof by reason of its very general nature is by no means easy to follow and it seems desirable

* *Proc. R.S.*, Vol. 79, pp. 182 et seq.

to investigate the probable error of this coefficient on the lines laid down by Pearson and Filon in their classic paper on the probable errors of frequency constants*.

Let ${}_3\rho_{12}$ be the correlation between variables 1 and 2 for a constant value of 3.

Then

$$ {}_3\rho_{12} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{1-r_{13}^2}\sqrt{1-r_{23}^2}}. $$

Taking logarithmic differentials, we find,

$$ \frac{d\,{}_3\rho_{12}}{{}_3\rho_{12}} = \frac{dr_{12}}{r_{12}-r_{13}r_{23}} + \frac{dr_{13}(r_{13}r_{12}-r_{23})}{(r_{12}-r_{13}r_{23})(1-r_{13}^2)} + \frac{dr_{23}(r_{23}r_{12}-r_{13})}{(r_{12}-r_{13}r_{23})(1-r_{23}^2)}. $$

Squaring, summing and dividing both sides by n, the total number of observations, and substituting for the standard deviation of the errors in r_{12}, r_{13}, r_{23}, and for the correlations of errors in those quantities the values given by Pearson and Filon in the paper already cited, we find after striking out the common factors $(1-r_{13}^2)(1-r_{23}^2)$

$$ \begin{aligned} &\frac{\Sigma^2\,{}_3\rho_{12}(r_{12}-r_{13}r_{23})^2(1-r_{13}^2)(1-r_{23}^2)}{{}_3\rho_{12}^2}\cdot n \\ &= (1-r_{12}^2-r_{13}^2-r_{23}^2+2r_{12}r_{13}r_{23})\{(1-r_{23}^2)(r_{23}-r_{12}r_{13})r_{12}r_{13} \\ &\qquad + (1-r_{13}^2)(r_{13}-r_{12}r_{23})r_{12}r_{23} - (r_{13}-r_{12}r_{23})(r_{23}-r_{12}r_{13})r_{13}r_{23}\} \\ &+ (1-r_{13}^2)(1-r_{23}^2)\{(1-r_{12}^2)+(r_{13}-r_{12}r_{23})^2+(r_{23}-r_{12}r_{13})^2-2r_{23}(1-r_{12}^2)(r_{23}-r_{12}r_{13}) \\ &\qquad - 2(1-r_{12}^2)(r_{13}-r_{12}r_{23})r_{13}+2(r_{13}-r_{12}r_{23})(r_{23}-r_{12}r_{13})r_{12}\}. \end{aligned} $$

But the second part of the expression to the right of the sign of equality reduces to

$$ (1-r_{13}^2)(1-r_{23}^2)(1-r_{12}^2)(1-r_{12}^2-r_{13}^2-r_{23}^2+2r_{12}r_{13}r_{23}), $$

and the second factor of the first part reduces to

$$ 1-r_{12}^2-r_{13}^2-r_{23}^2+2r_{12}r_{13}r_{23}-(1-r_{12}^2)(1-r_{13}^2)(1-r_{23}^2), $$

so that the whole expression on the right reduces at once to

$$ (1-r_{12}^2-r_{13}^2-r_{23}^2+2r_{12}r_{13}r_{23})^2. $$

But

$$ {}_3\rho_{12}^2 = \frac{(r_{12}-r_{13}r_{23})^2}{(1-r_{13}^2)(1-r_{23}^2)}, $$

so that

$$ \Sigma\,{}_3\rho_{12} = \frac{1-r_{12}^2-r_{13}^2-r_{23}^2+2r_{12}r_{13}r_{23}}{\sqrt{n}\,(1-r_{13}^2)(1-r_{23}^2)} $$

$$ = \frac{1-{}_3\rho_{12}^2}{\sqrt{n}}, $$

and the probable error of ${}_3\rho_{12}$ is

$$ = \frac{\cdot 67449\,(1-{}_3\rho_{12}^2)}{\sqrt{n}}, $$

and is thus, as stated by Yule, of the same form as the probable error of the direct correlation between any two variables.

* "On the Probable Errors of Frequency Constants and on the Influence of Random Selection on Variation and Correlation." By Karl Pearson, F.R.S., and L. N. G. Filon, B.A., *Phil. Trans. R.S.*, A, Vol. 191, pp. 229 et seq.

BOOK NOTES AND BIBLIOGRAPHY.

BOOK NOTES.

Interpolationsrechnung, by T. N. THIELE. Leipzig. B. G. Teubner, 1909, M. 10.

This interesting work which is divided into four chapters begins by evolving the general interpolation formula

$$X = A + (x-a)\,[\delta'(a,\ b) + (x-b)\,\{\delta''(a, \ldots,\ c) + (x-c)\,\{\delta'''(a, \ldots,\ d) + \ldots\}\}]$$

where $$\delta'(x,\ a) = \frac{X-A}{x-a} \text{ and } \delta''(x, \ldots, b) = \frac{\delta'(x, a) - \delta'(a, b)}{x-b}, \text{ etc.}$$

This formula does not depend as Newton's formula does on the tabulation of the function for even differences in the argument and in its practical application one differences the function in the usual way and then divides the differences so obtained by the corresponding difference in the argument. (Cp. De Morgan's *Differential and Integral Calculus*, Ch. XVIII., on Interpolation and Summation.) The remainder of the first chapter deals with Newton's Method and with series and the problems arising from them. The second chapter is devoted to symbolic work and although helpful and interesting in places it is rather overburdened with notation, and the third chapter gives auxiliary methods such as the graphic and explains the use of a "qualifizirte Differenz" which is based on expressions formed symbolically, $(E-a)f(x) = f(x+1) - af(x)$ for the first difference and $(E^2 - 2cbE + b^2)f(x) = f(x+2) - 2cbf(x+1) + b^2 f(x)$ for the second difference and so on. These differences are applied in connection with exponential and periodic functions. The last chapter of the book deals with interpolation when there is more than one independent variable, but contains no new suggestions.

Interpolation is perhaps chiefly serviceable in its arithmetical applications and we can recommend the present work not only for its examples on this part of the subject but also for its interesting theoretical treatment.

W. P. E.

The Theory of the Construction of Tables of Mortality. A course of Lectures by G. F. HARDY, F.I.A., Delivered during the Session 1904—5. Published for the Institute of Actuaries by C. and E. Layton, 1909.

These Lectures which were intended mainly for Actuaries deal with graduation and the problems arising from it. They recount briefly the older methods of graduation such as the Graphic and Woolhouse's Difference Method but are chiefly concerned with the fitting of curves and with the practical use of Makeham's hypothesis for graduating a mortality Table.

It is a pity that owing to the omission of those parts of the subject with which his audience was familiar and to the fact that Mr Hardy has not had time to enlarge what were notes for

a larger work the Lectures in their published form appear a little disjointed. This is unfortunate as it gives an impression of hurry which is enhanced by the fact that there is neither an index nor list of contents!

To biometricians Mr Hardy's treatment of frequency curves will not we think pass without a certain amount of criticism. On p. 39 the differential equation from which the Pearson-type curves are evolved is given as

$$\frac{1}{y}\cdot\frac{dy}{dx}=\frac{bx-x^2}{a-bx+cx^2},$$

which does not seem to lead to Type I, while the curves have been renumbered simply according to range which seems unsatisfactory, as, if we are to give up a numbering to which we are accustomed, it is well to adopt one which will not call for revision. This could have been done either according to the number of terms in the denominator of the right-hand side of the fundamental equation or according to the number of moments required, e.g. the curves requiring two moments numbered II a, II b, etc., and then further curves could be added if for any class of statistics a further term had to be included in the differential equation.

There are one or two other details with which we find it hard to agree; for example, on p. 46, Type III should we think have been used to fit the binomial series, while we find it hard to follow Mr Hardy's statement that Types III and IV are not specially useful to Actuaries, the reason given is hardly convincing, and we have found both curves in not a few cases from actuarial data.

To most people interested in graduation however the part of Mr Hardy's book which will be most attractive will certainly be his account of his own graduation of the select tables (i.e. tables giving the effect of medical selection) based on the new British Offices experience. Prior to this graduation there had we believe been no published graduation of a select table by any curve-fitting process, and though the two diagrams (pp. 74 and 75) do not betoken a very close fit it must be borne in mind that it is on the sum of the two curves that the agreement really depends, and little fault can be found with the resulting graduation. But apart from this one cannot expect a perfect method of graduation to be reached immediately, and Mr Hardy has probably done far more in actuarial circles for scientific graduation than most actuaries realise.

Our disagreement with some of the remarks made in Mr Hardy's book does not obscure but rather tends to accentuate in our minds the value of many of his actuarial suggestions, while we hope his work will stimulate other Actuaries to a study of general curve-fitting, so that the old methods which have been used so freely in the past may be displaced by those which Mr Hardy more than any other actuary has brought before the notice of his profession.

W. P. E.

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